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ABSTRACT

The Summer Training and Education Program (STEP) was developed in 1984 as a strategy for reducing the number of young people who leave school without the skills and motivation necessary for productive employment. The STEP intervention involves collaboration between the public schools and the federal Summer Youth Employment and Training Program (SYETP), which provides more than 650,000 low-income teenagers throughout the country with eight weeks of employment every summer under the auspices of the Job Training Partnership Act. With summer income as an incentive, 14- and 15-year-olds who are doing poorly in school and are eligible for SYETP enroll in STEP for 15 months--two summers of half-time work and half-time basic skills remediation and life skills instruction, and one school year of supportive services. The goal is to reduce the learning losses typically experienced by disadvantaged youth during the summer vacation, improve reading and math skills, increase graduation rates, and reduce the incidence of early parenthood. STEP has been operating at the following sites: (1) Boston, Massachusetts; (2) Fresno, California; (3) San Diego, California; (4) Portland, Oregon; and (5) Seattle, Washington. This, the third annual research report on the 4-year demonstration of STEP, records its progress through fall 1987 and describes its impact on three cohorts of 320 enrollees at each of the five sites (4,800 total), at the various points of their participation they had reached at that time. Numerous tables and figures illustrate the data. Appendices provide the following: (1) program participation analysis; (2) tables referenced in text; (3) control group losses; and (4) technical data. (BJV)

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Summer Training and Education Program (STEP)

Report on the 1987 Experience

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Summer 1988

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Summer Training and Education Program (STEP)

Report on the 1987 Experience

by Cynthia L. Sipe
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Summer 1988

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CONTENTS

Page

EXECUTIVE SUMMARY i

I. INTRODUCTION.................................. 1

- BACKGROUND.................................. 3
- POLICY ISSUES.................................. 6
- ORGANIZATION OF THIS REPORT.................................. 9

II. THE STEP MODEL AND EARLY FINDINGS.................................. 11

- TARGET POPULATION.................................. 11
- PROGRAM COMPONENTS.................................. 12
- SITE DESCRIPTIONS.................................. 15
- EARLY PROGRAM FINDINGS.................................. 17

III. RESEARCH DESIGN.................................. 21

- IMPLEMENTATION ANALYSIS.................................. 21
- IMPACT ANALYSIS.................................. 22
- DESIGN STRATEGY.................................. 23
- DATA COLLECTION STRATEGY.................................. 25
- ANALYTIC STRATEGIES.................................. 31

IV. PROGRAM IMPLEMENTATION.................................. 35

- YEAR THREE.................................. 36
- PARTICIPANT SELECTION AND ASSESSMENTS.................................. 38
- THE REMEDIATION COMPONENT.................................. 44
- THE LIFE SKILLS AND OPPORTUNITIES (LSO) COMPONENT.. 56
- THE WORK EXPERIENCE COMPONENT.................................. 64
- THE SCHOOL-YEAR SUPPORT (SYS) COMPONENT.................................. 70
- SUMMARY.................................. 76

V. PARTICIPANT CHARACTERISTICS AT BASELINE.................................. 79

- THE COHORT III SAMPLE.................................. 79
- COHORT II TREATMENT RETURNEES.................................. 92
- COHORT II CONTROL RETURNEES.................................. 99
- THE WAVE I FOLLOW-UP INTERVIEW SAMPLE.................................. 99
- SUMMARY OF PARTICIPANT CHARACTERISTICS..................................104

VI. FIRST-SUMMER IMPACTS--COHORT III AND AGGREGATE SAMPLES..................................107

- ACADEMIC PERFORMANCE..................................108
- LIFE SKILLS OUTCOMES..................................126

CONTENTS - (cont'd)

Page

VII. SCHOOL-YEAR AND SECOND-SUMMER IMPACTS FOR COHORT II...137

SCHOOL DATA ANALYSIS...137
CHARACTERISTICS OF COHORT II RETURNEES...151
DESCRIPTION OF SECOND-SUMMER AND 15-MONTH OUTCOMES...154

VIII. POSTPROGRAM IMPACTS FOR COHORT I...171

EDUCATIONAL OUTCOMES...172
FERTILITY-RELATED OUTCOMES ...180
EMPLOYMENT OUTCOMES...185
SUMMARY OF POSTPROGRAM OUTCOMES...187

IX. COST ANALYSIS...189

METHODOLOGY...191
THE COSTS OF STEP...198
TOTAL 15-MONTH NET COSTS ...208
OTHER COST ESTIMATES...209

X. SUMMARY AND CONCLUSIONS...211

STEP IMPLEMENTATION DURING THE THIRD SUMMER...211
PARTICIPANT CHARACTERISTICS...212
SUMMER IMPACTS...213
SCHOOL-YEAR IMPACTS FOR COHORT II...214
SECOND-SUMMER RESULTS FOR COHORT II...215
POSTPROGRAM IMPACTS FOR COHORT I...216
COSTS...217
IMPLICATIONS...218

REFERENCES ...221

APPENDIXES

A. PROGRAM PARTICIPATION ANALYSIS ...225

B. TABLES REFERENCED IN TEXT ...227

C. CONTROL GROUP LOSSES ...253

D. TECHNICAL APPENDIXES ...259

TABLES

Page

III.1 SUMMER TRAINING AND EDUCATION PROGRAM DATA BASE AND SOURCE 26

IV.1 LOCAL OPERATING AGENCIES 37

IV.2 ACADEMIC ELIGIBILITY MEASURES BY SITE 39

IV.3 REENROLLMENT OF COHORT II YOUTH IN 1987 41

IV.4 YOUTH TESTED BY SITE AND COHORT 45

IV.5 SUMMER TRAINING AND EDUCATION PROGRAM SAMPLE WEEKLY SCHEDULE 46

IV.6 REMEDIATION TEACHER CHARACTERISTICS BY SITE 51

IV.7 MEAN HOURS (AND PERCENTAGE) OF PARTICIPATION IN CLASSROOM AND WORK EXPERIENCE COMPONENT 57

IV.8 LSO TEACHER CHARACTERISTICS BY SITE 61

IV.9 JOB CLASSIFICATION FOR COHORT II AND III YOUTH BY SITE 67

IV.10 SUMMARY OF SCHOOL-YEAR SUPPORT CONTACTS BY SITE 73

V.1 SELECTED CHARACTERISTICS OF THE RANDOMIZED AND LEARNING ANALYSIS SAMPLES FOR COHORT III 81

V.2 DEMOGRAPHIC AND ECONOMIC CHARACTERISTICS OF COHORT III 82

V.3 EDUCATIONAL ABILITY AND SCHOOL PARTICIPATION CHARACTERISTICS OF COHORT III 86

V.4 FERTILITY-RELLATED CHARACTERISTICS OF COHORT III 90

V.5 DEMOGRAPHIC AND ECONOMIC CHARACTERISTICS OF THE COHORT II RETURNING YOUTH 94

V.6 EDUCATIONAL ABILITY AND SCHOOL PARTICIPATION CHARACTERISTICS OF COHORT II RETURNING TREATMENTS 97

TABLES - (cont'd)

Page

V.7 FERTILITY-RELATED CHARACTERISTICS OF COHORT II RETURNING TREATMENTS 100

V.8 COMPARISON OF THE WAVE I INTERVIEW SAMPLE TO THE ORIGINAL SAMPLE ON SELECTED CHARACTERISTICS.. 103

VI.1 MEANS OF PRETEST TO POSTTESST CHANGES IN READING AND MATH SCORES BY GENDER AND RACE/ETHNICITY FOR COHORT III 110

VI.2 MEANS OF PRETEST TO POSTTEST CHANGES IN READING AND MATH SCORES BY SITE FOR COHORT III 111

VI.3 NET IMPACT OF PARTICIPATION IN STEP ON MAT SCALED SCORES IN READING AND MATH FOR COHORT III AND THE AGGREGATE SAMPLE OF COHORTS II AND III .. 113

VI.4 NET IMPACT OF PARTICIPATION IN STEP ON MAT SCALED SCORES IN READING AND MATH BY REMEDIATION SUBGROUP IN BOSTON AND SAN DIEGO--COHORT III 118

VI.5 COHORT III YOUTH WHO REENROLLED IN SCHOOL, FALL 1986, OVERALL AND BY GENDER, RACE/ETHNICITY AND SITE 125

VI.6 NET IMPACT OF PARTICIPATION IN STEP ON BIRTH CONTROL KNOWLEDGE VARIABLES FOR COHORT III BY GENDER, RACE/ETHNICITY, AND SITE 128

VI.7 BASELINE TO ENDLINE CHANGES IN THE PERCENTAGES OF COHORT III YOUTH WITH SEXUAL EXPERIENCE, THOSE HAVING SEX IN THE PREVIOUS TWO MONTHS, AND USING CONTRACEPTIVES AT LAST INTERCOURSE 132

VI.8 ESTIMATED IMPACTS OF STEP ON CONTRACEPTIVE USE AMONG SEXUALLY ACTIVE YOUTH IN COHORT II AND THE AGGREGATE SAMPLE OF COHORTS II AND III 135

TABLES - (cont'd)

Page

VII.1 THE ESTIMATED IMPACTS OF STEP ON ABSENTEEISM AND TEST SCORESS IN READING AND MATH DURING THE INTERVENING SCHOOL YEAR BY GENDER, RACE/ ETHNICITY AND SITE FOR COHORT II 144

VII.2 THE ESTIMATED IMPACTS OF STEP ON SCALED TEST SCORES DURING THE INTERVENING SCHOOL YEAR BY SITE FOR COHORT II 146

VII.3 THE ESTIMATED IMPACTS OF STEP ON CREDITS EARNED AND GRADE PROMOTION DURING THE INTERVENING SCHOOL YEAR BY GENDER, RACE/ETHNICITY AND SITE FOR COHORT II 149

VII.4 SELECTED CHARACTERISTICS OF RETURNING COHORT II TREATMENT YOUTH AT BEGINNING OF SECOND SUMMER BY SITE 153

VII.5 SELECTED CHARACTERISTICS OF RETURNING COHORT II YOUTH IN SEATTLE AND SAN DIEGO 155

VII.6 CHANGES IN MEAN READING AND MATH SCORES OVER 15 MONTHS FOR RETURNING COHORT II TREATMENTS AND SAN DIEGO AND SEATTLE CONTROLS 157

VII.7 CHANGES IN MEAN KNOWLEDGE OF CONTRACEPTIVE METHODS AND AVAILABILITY OVER 15 MONTHS FOR RETURNING COHORT II TREATMENTS AND SAN DIEGO AND SEATTLE CONTROLS 162

VII.8 NET IMPACT OF PARTICIPATION IN STEP ON CONTRACEPTIVE KNOWLEDGE FOR COHORT I OVERALL AND BY GENDER, RACE/ETHNICITY AND SITE 164

VII.9 BASELINE TO FOLLOW-UP CHANGE IN THE PERCENTAGE OF COHORT II YOUTH WITH SEXUAL EXPERIENCE AND USING CONTRACEPTIVES AT MOST RECENT INTERCOURSE (BY GENDER)....................................166

VII.10 BASELINE TO ENDLINE CHANGES IN MEAN HEALTH, DRUG AND APPROPRIATE WORK BEHAVIOR KNOWLEDGE FOR COHORT II 168

TABLES - (cont'd)

Page

VIII.1 THE ESTIMATED IMPACTS OF STEP ON ABSENTEEISM AND TEST SCORES IN READING AND MATH DURING FIRST POSTPROGRAM SCHOOL YEAR BY GENDER, RACE/ETHNICITY AND SITE FOR COHORT I 173

VIII.2 THE ESTIMATED IMPACTS OF STEP ON SCALES TEST SCORES DURING THE FIRST POSTPROGRAM SCHOOL YEAR BY SITE FOR COHORT I 175

VIII.3 THE ESTIMATED IMPACTS OF STEP ON CREDITS EARNED, GRADE, AND PROMOTION DURING THE FIRST POSTPROGRAM SCHOOL YEAR BY GENDER, RACE/ETHNICITY AND SITE FOR COHORT I 176

VIII.4 THE ESTIMATED IMPACTS OF STEP ON DROPPING OUT OF SCHOOL BY THE END OF THE FIRST POSTPROGRAM YEAR BY GENDER, RACE/ETHNICITY AND SITE FOR COHORT I 178

VIII.5 NET IMPACT OF PARTICIPATION IN STEP ON CONTRACEPTIVE KNOWLEDGE FOR COHORT I OVERALL AND BY GENDER, RACE/ETHNICITY AND SITE 182

VIII.6 BASELINE TO FOLLOW-UP CHANGE IN THE PERCENTAGE OF COHORT I YOUTH WITH SEXUAL EXPERIENCE AND USING CONTRACEPTIVES AT LAST INTERCOURSE BY GENDER 183

VIII.7 THE ESTIMATED IMPACT OF STEP ON EARNINGS, THE PERCENTAGE EMPLOYED AND WEEKLY WAGE IN QUARTER NINE BY GENDER, RACE/ETHNICITY AND SITE FOR COHORT I.. 186

IX.1 SUMMARY OF STEP COSTS 190

IX.2 THE SYNTHETIC COHORT APPROACH: SOURCES OF COMPONENT COST 193

IX.3 ESTIMATED NET SOCIAL COSTS PER COHORT MEMBER BY SITE FOR 15 MONTHS OF STEP 199

IX.4 NET OUT-OF-POCKET COSTS FOR COHORT MEMBER BY SITE FOR 15 MONTHS OF STEP 201

EXECUTIVE SUMMARY

STEP, the Summer Training and Education Program, was developed by Public/Private Ventures (P/PV) in 1984 as a strategy for reducing the number of young people who leave school without the skills and motivation necessary for productive employment. This, the third annual research report on the four-year demonstration, records its experience through Fall 1987 and describes its impacts on three cohorts of enrollees at the various points they had reached at that time.

Overall demonstration management is the responsibility of P/PV, a private not-for-profit Philadelphia corporation that seeks effective approaches to job training and education for economically disadvantaged people. Funding is provided by Aetna Life and Casualty Foundation, The Ahmanson Foundation, The Edna McConnell Clark Foundation, The Ford Foundation, W.T. Grant Foundation, The William and Flora Hewlett Foundation, The Robert Wood Johnson Foundation, Lilly Endowment, James C. Penney Foundation, the U.S. Department of Labor and the U.S. Department of Health and Human Services.

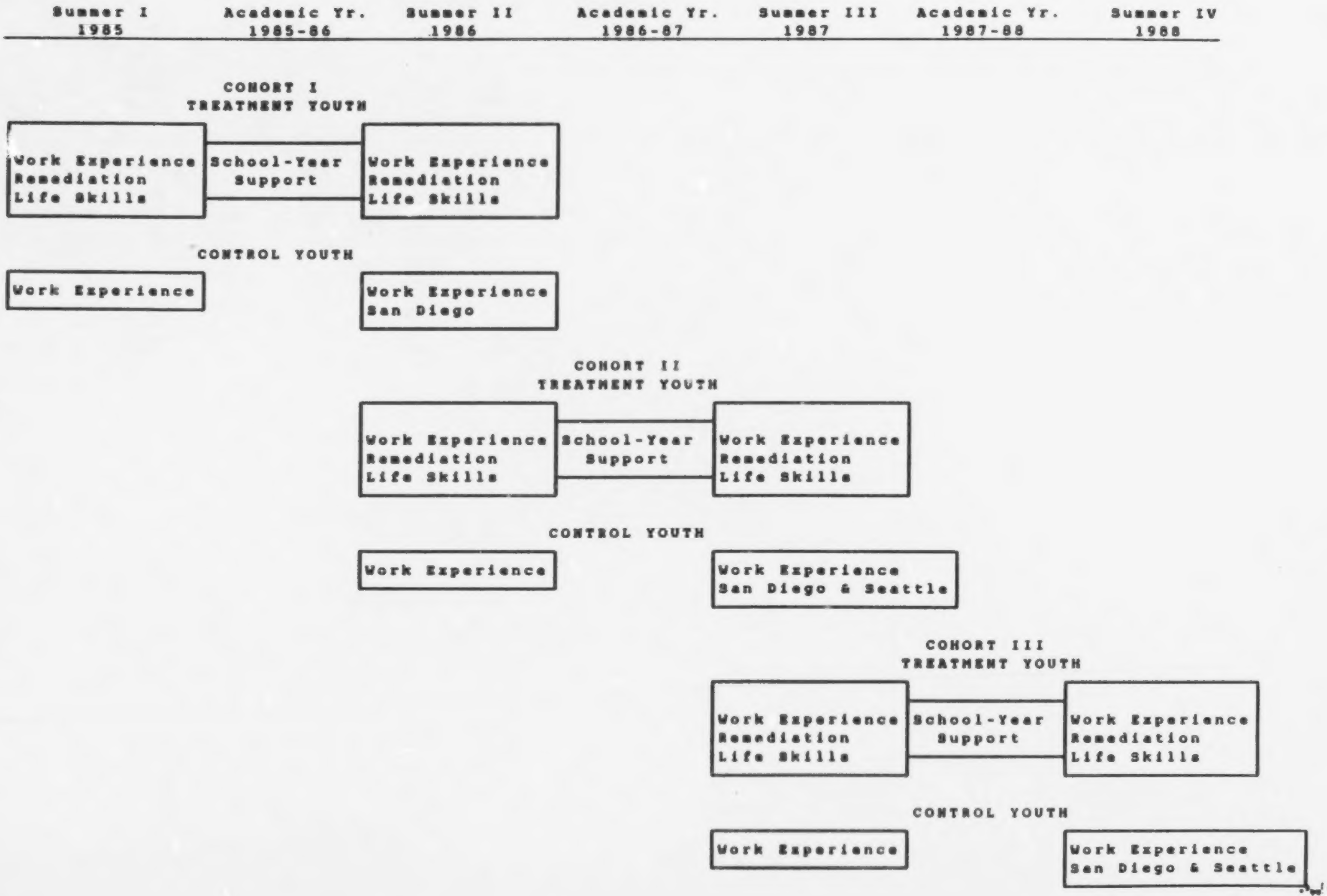
The STEP intervention is a preventive one and involves collaboration between the public schools and the federal Summer Youth Employment and Training Program (SYETP), which provides more than 650,000 low-income teenagers throughout the country with seven to eight weeks of employment every summer under the auspices of the Job Training Partnership Act (JTPA). Since 1987, localities have been required by JTPA to offer remedial assistance to SYETP enrollees whose test scores show that their basic skills are weak. Therefore, the STEP experience is particularly timely and instructive.

With summer income as an incentive, 14- and 15-year-olds who are doing poorly in school and are eligible for SYETP enroll in STEP for 15 months--two summers of half-time work and half-time basic skills remediation and life skills instruction, and one school year of supportive services. (See Figure 1.) The goal is to reduce the learning losses typically experienced by disadvantaged youth during the long summer vacation, improve reading and math skills, increase graduation rates and reduce the incidence of teen parenthood, which together with poor academic performance is a major cause of dropping out of school.

Following a successful three-city pilot during the summer of 1984, P/PV began a national demonstration of STEP in 1985 in five cities: Boston, Massachusetts; Fresno and San Diego, California; Portland, Oregon; and Seattle, Washington. To evaluate the impacts of the program, half the eligible youth interested in participating were randomly assigned to the treatment group and

Figure 1

SUMMER TRAINING AND EDUCATION PROGRAM (STEP) OPERATIONAL PLAN


Summer I 1985
Academic Yr. 1985-86
Summer II 1986
Academic Yr. 1986-87
Summer III 1987
Academic Yr. 1987-88
Summer IV 1988
COHORT I
TREATMENT YOUTH
Work Experience
Remediation
Life Skills
School-Year Support
Work Experience
Remediation
Life Skills
CONTROL YOUTH
Work Experience
Work Experience
San Diego
COHORT II
TREATMENT YOUTH
Work Experience
Remediation
Life Skills
School-Year Support
Work Experience
Remediation
Life Skills
CONTROL YOUTH
Work Experience
Work Experience
San Diego & Seattle
COHORT III
TREATMENT YOUTH
Work Experience
Remediation
Life Skills
School-Year Support
Work Experience
Remediation
Life Skills
CONTROL YOUTH
Work Experience
Work Experience
San Diego & Seattle

offered STEP program services; the remaining half were assigned to the control group and guaranteed an SYETP job. In San Diego and Seattle, control youth were offered a job for two summers; in the other sites, control youth were offered an SYETP job for their cohort's first summer only. All youth are paid minimum wage for their participation, both on the job and in class.

At the sites, day-to-day management and operation of the program are the responsibility of employment and training agencies, school districts and a number of other local institutions. Local program operation costs are covered primarily by Title IIB (SYETP) funds under JTPA and local school district resources.

In each of the five sites, three cohorts of approximately 320 youth each are participating in both the program and the research study, for a total of 4,800 youth in the demonstration. Research will continue through the conclusion of the operational phase at the end of August 1988 and will follow the youth through 1992, six months beyond the last enrollees' scheduled date of graduation.

PRINCIPAL FINDINGS

The third annual STEP report records the experience of all three cohorts at different points in their participation. The findings are based on four groups of data: pre- and post-summer Metropolitan Achievement Test scores and STEP questionnaire responses, school records and postprogram interviews.

First-Summer Outcomes for Cohort III

The third cohort (Cohort III) of 750 treatment youth and 750 control youth entered STEP in the summer of 1987. This report records outcomes from their initial seven weeks in the program.

Like the first two cohorts of STEP youth, Cohort III youth are at risk of dropping out of school and of becoming adolescent parents. Most are enrolled in the eighth and ninth grades but perform well below grade level on the Metropolitan Achievement Test, averaging a sixth grade level in reading and a seventh grade level in math at baseline. (See Figure 2.) Their school records reveal sporadic attendance patterns and 33 percent had been held back in one or more grades. All meet the eligibility requirements of JTPA. The high percentage of youth who are already sexually experienced (44 percent) are at risk for teen pregnancy and thus an increased likelihood of dropping out of school before graduation.

Cohort III was the first STEP group to achieve significant gains in both reading (0.3 of a grade) and math (0.2 of a grade) in their first summer of the program. (See Figures 3 and 4.) The

Figure 2
Mean Grade Completed and MAT Test Scores at Baseline

COHORT III

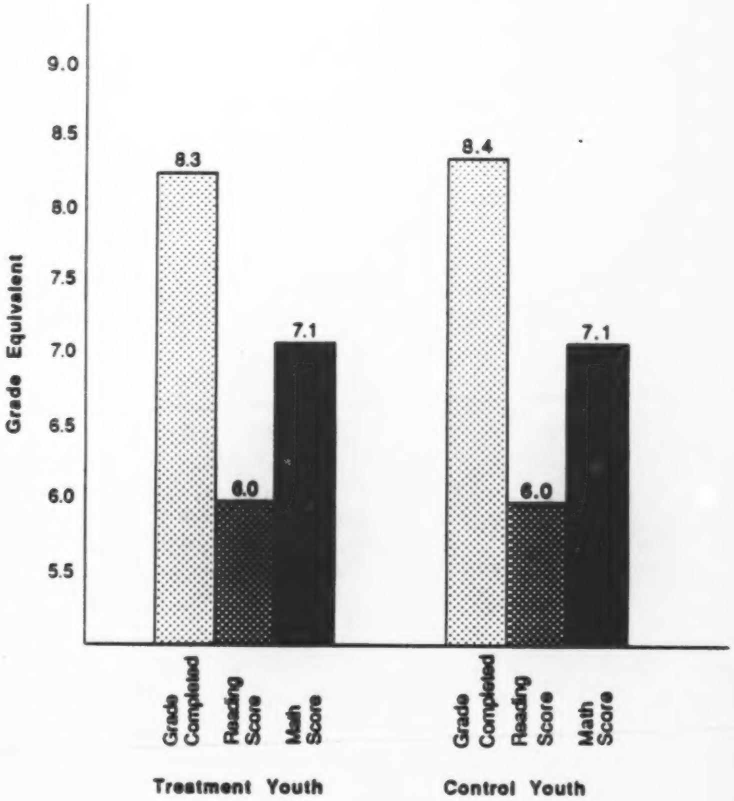


Figure 3
Mean MAT Reading Scores at Pretest and Posttest (in Grade Equivalents)

COHORT III

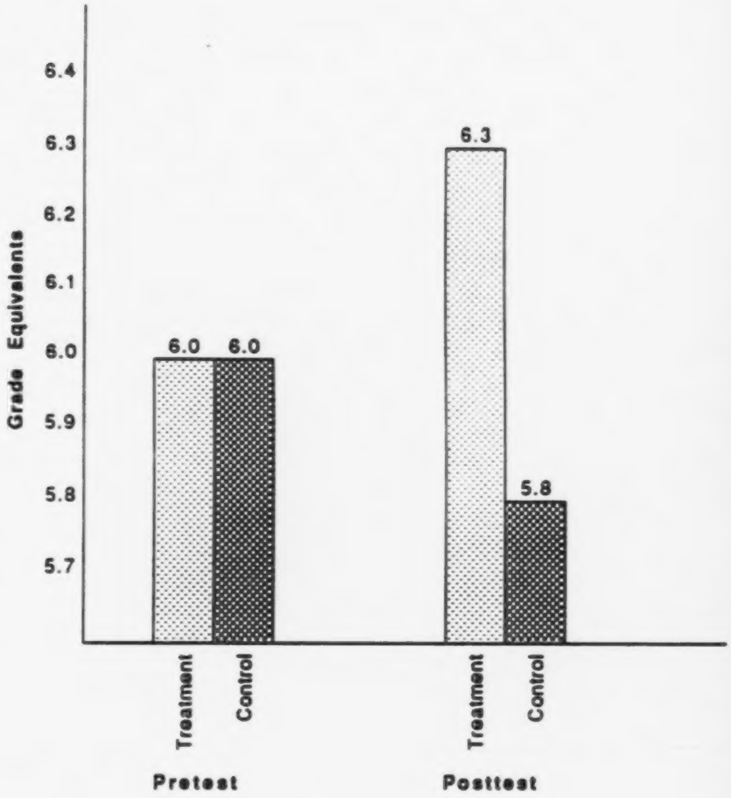
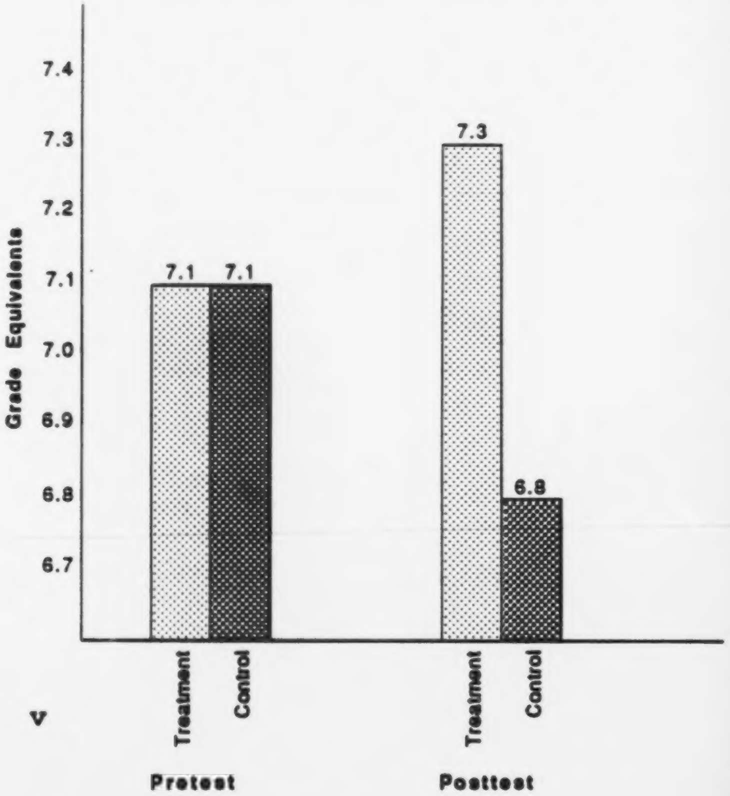


Figure 4
Mean MAT Math Scores at Pretest and Posttest (in Grade Equivalents)

COHORT III



control group continued to experience significant learning losses, though they were of lesser magnitude than those seen for previous cohorts. Nevertheless, the net impact of STEP participation continued to be significant, with treatment youth scoring 0.5 of a grade higher in reading and 0.6 of a grade higher in math than did control youth.

These effects were widespread; they were significant for both males and females and in all sites but one. Gains in math were significant for all ethnic groups except Asians, who started the program with math scores appropriate to their grade levels; impacts on reading scores were significant for blacks, whites and others, but not for Asian and Hispanic youth. Among Asians, both treatments and controls gained; the difference between the groups was not significant.

Knowledge of contraceptive methods and their availability increased significantly for all groups in all sites, but there was less behavioral impact than that reported by Cohort II at the end of their first summer of STEP. This may be due in part to Cohort III females' reported rate of contraceptive use at baseline being 40 percent higher than that of Cohort II females, leaving less room for improvement.

Mid-Program and 15-Month Outcomes for Cohort II

The second group of youth, Cohort II, entered STEP in the summer of 1986, were offered support activities during the 1986-87 school year, and returned for a second summer of STEP in the summer of 1987. The second annual STEP report recorded their very strong outcomes in all sites and among all gender and racial/ethnic groups during the first summer of participation. This report records their experience during the school year and their final summer of STEP, and over the whole 15-month period.

Examination of school data--on attendance, reading and math test scores, credits earned, grade promotion and dropout behavior--reveals no overall net impact of STEP on the performance of STEP youth relative to controls during the school year following the first summer of participation. Among subgroups, Hispanic treatment youth had better attendance than Hispanic controls, and youth who had the most contact with the STEP school-year support (SYS) component earned significantly more credits than did controls and were more likely to be promoted.

During Cohort II's second summer, treatment youth experienced no significant changes in either reading and math, but control youth in the two sites where their performance was measured continued to experience substantial losses--1.1 grade levels in reading and 0.6 grade levels in math. As a result, the net impact of STEP participation in these two sites (Seattle and San Diego) is equivalent to 0.7 of a grade in reading and .8 of a grade in math. We assume that control youth's summer learning losses and treatment youth's net gains throughout the demonstration approximate those seen in Seattle and San Diego.

Treatment youth retained much of the knowledge of contraception they acquired during the first summer and continued to increase this knowledge during the second, but effects on behavior are more difficult to gauge. At the end of Cohort II's first summer, treatment youth were 53 percent more likely than controls to report use of contraception; at the end of their second summer, reported rates of contraceptive use were not different for treatments and controls.

Over 15 months of STEP, treatment youth gained in both reading (0.3 of a grade) and math (a full grade level). Again the experience in Seattle and San Diego, where control youth were tested during a second summer of SYETP employment, provides some insight into the relative experience of treatments and controls in the demonstration. Since controls continued to lose ground during the summer, though not during the school year, the net impact of 15 months of STEP was equivalent to 0.9 of a grade in reading and 0.7 of a grade in math.

Postprogram Outcomes for Cohort I

The first cohort of youth entered STEP in the summer of 1985. Treatment youth had their first summer of instruction and work experience in 1985, participated in School Year Support (SYS) activities during the academic year 1985-86, and returned to complete the 15-month program in the summer of 1986. In-program outcomes for Cohort I were covered in earlier STEP annual reports. This report records the postprogram experiences of Cohort I treatment and control youth, most of whom were in 10th grade during their first postprogram year (1986-87) and 11th grade at the time of follow-up interviews during the next semester in Fall 1987.

School data for the school year following participation in the 15-month program show that STEP youth had higher math scores than controls and were more likely to be promoted. No significant net impacts were detected on reading scores, attendance and suspension rates, the number of earned credits, level of schooling achieved, or rates of employment and wage levels during the summer of 1987. Dropout rates for STEP youth were lower than those of controls, though the total numbers were not large enough for the differences to be statistically significant.

The strongest net impacts on dropout rates were seen in the three subgroups that had profited the most from their first summer of participation: youth in the Boston program, and Hispanic enrollees and females in all sites. Impacts on math scores were strongest among black youth, who also had strong math impacts during the first summer.

Follow-up interviews during the Fall 1987 semester--14 to 17 months after STEP participation concluded--disclosed that Hispanic participants continued to show the strongest impacts: STEP participation significantly reduced their dropout rates.

The follow-up process also disclosed that STEP youth maintained the higher levels of knowledge on how to avoid pregnancy they had gained in the program; control youth had not increased this knowledge at all. However, participation in STEP did not affect reported rates of sexual initiation, recent sexual activity or the incidence of pregnancy.

PROGRAM IMPLEMENTATION

Operations

Site experience and familiarity with the STEP model and its operational requirements resulted in continuing improvements in implementation in the demonstration's third year. Operations, curriculum and instruction, and SYS activities--all of which were greatly strengthened for the second year of the demonstration--continued to improve.

Students attended classroom activities an average of 85 percent of available time. Employers have become familiar with the program and are no longer resistant to the idea of hiring younger youth who are available only part time. All sites operated SYS programs in 1987 and greater uniformity was achieved in the level of contact with youth. However, critical tutoring services were found to be difficult to provide because of transportation and scheduling problems, the unavailability of tutors and the failure of youth to acknowledge that they need additional help.

Costs

The research report presents an analysis of both STEP's social costs, which include the value of contributed goods and services but exclude youth wages since they are transfer payments, and out-of-pocket costs, which exclude the value of in-kind contributions and include wages.

The estimated cost of operating a 15-month STEP program beyond what it costs to pay youth wages and administer the basic SYETP program is $1525 per enrollee--a sum that can be reduced to $1370 on average by the utilization of contributed goods and services. The $1370 is an average for the demonstration sites and includes developmental and first summer costs of remediation ($470), the life skills component ($86) and program administration ($40). The second summer costs about 75 percent of this figure, plus reenrollment costs and SYS costs ($298). All demonstration sites secured contributions of space and equipment.

CONCLUSIONS

The qualitative and quantitative research to date leads to the following conclusions:

1. STEP is an effective short-term intervention. It has proven feasible to implement, is well-targeted on a high-risk group of youngsters, and has produced effective near-term outcomes. Although there are some anomalous patterns, the program's net impacts on reading and math test measures are strong and consistent; and STEP has proven capable of significantly raising participating youth's knowledge of contraception.

2. The program's most convincing effects are in stemming academic loss and sharply increasing knowledge of contraception and sexuality. Although STEP participants showed modest positive gains in reading and math test scores, net impact findings were driven by the large declines experienced by control group members. Thus, the program's capacity to stem these losses, as well as its large measured effects on treatment youth's knowledge of contraception, were its strongest measurable results.

3. The research documents the phenomenon of learning loss among target youth; this issue appears worthy of further work and exploration. The net impacts on learning produced by the STEP program were principally a function of pre- or posttest score declines among control youth in the sample. Treatments presumably would, absent participation in STEP, have experienced similar losses.

Thus, it seems reasonable to conclude that the learning loss phenomenon is a real one. Our findings are consistent with prior research, but this remains an area in which further research is needed to clarify and elucidate the findings that have been produced. In view of the significance of these findings for disadvantaged youth, there is clear value in understanding this phenomenon better.

4. Preliminary findings regarding STEP's long-term impacts are small and mixed. The results thus far are consistent with the hypothesis that STEP reduces dropout behavior, but are based on small numbers and are not statistically significant. Consistent effects on fertility-related behavior have not been detected. These are, however, early findings. Only when a longer-term record of results has been compiled will it be possible to make conclusive statements about STEP's long-term effects on participants.

5. STEP establishes a strong need for increased and intensive interventions aimed at bettering the academic performance and sexual responsibility of at-risk youth. The poor initial test levels among all youth targeted for STEP, their high risk of teenage pregnancy and the documented learning loss experienced by controls all indicate the seriousness of the problem that disadvantaged youth face. Moreover, STEP's strong positive results indicate that these deficiencies are susceptible to programmatic intervention and that the summer is an effective point for such an intervention.

These findings raise a host of related issues: how to better sustain--or even increase--the summer's results; how to better integrate summer academic/work programs with school-year curricula; and how to address the broader issue of year-round learning. Most clearly, however, STEP's results underscore the need for greater efforts to improve the school performance of disadvantaged youth, and the positive effects such efforts can yield.

6. STEP's short-term results, its operational success and the continuing need for such interventions make it a suitable candidate for broader replication. STEP was explicitly designed to capitalize on the federally funded Summer Youth Employment and Training Program (SYETP), which annually reaches some 600,000 youth in every part of the U.S. Recent federal amendments to the JTPA legislation, which mandate provision of remedial instruction as part of these summer employment programs, provide additional impetus to replication of a program like STEP, whose key feature is the integration of work and remediation.

Finally, STEP's potential as a successful summer intervention, documented by the research, underscores the value of its replication in full two-summer, 15-month form. The second summer's remediation and life skills components produce measurable effects, and the SYS program provides both a link between the two summers and continuous contact and support for STEP participants. The extent of learning loss among participants and STEP's capacity to both halt that loss in the short-term and actually produce academic gains argue strongly for STEP's wider adoption as one tool to prepare disadvantaged youngsters academically for productive roles in the workplace.

I. INTRODUCTION

The Summer Training and Education Program (STEP), an intervention designed to help reduce dropout levels among poor and educationally deficient youth, focuses on two factors that are closely associated with dropping out: poor academic performance and adolescent parenthood. Through a mix of work experience, basic skills remediation, and life skills and opportunities (LSO) instruction, STEP seeks to improve reading and math skills, increase the graduation rate and reduce the incidence of teen parenthood among disadvantaged youth who are doing poorly in school.

STEP targets 14- and 15-year-old youth who are at risk of dropping out of school. All participants are low-income and are performing below grade level in either reading or math. Over a 15-month period, the program offers these youth two consecutive summers of remediation, life skills instruction and work experience combined with various support services, such as counselor/advocates and group meetings offered during the intervening school year. During the summers, participants spend half their program hours attending remediation and life skills classes, and half their time in work experience.

Following a successful three-city pilot during the summer of 1984, a national demonstration of STEP began in Summer 1985 in five cities--Boston, Massachusetts; Fresno, California; Portland, Oregon; San Diego, California; and Seattle, Washington. To evaluate the impacts of the program, half the eligible youth interested in participating were randomly assigned to the treatment group and offered STEP program services; the remaining half were assigned to the control group and offered a summer job through the federal Summer Youth Employment and Training Program (SYETP). All youth are paid minimum wage for their participation, whether in class or on the job. In San Diego and Seattle, control youth were offered a job during both summers of the program; in the remaining sites, control youth were offered a job during the first summer only.

Overall management of the demonstration is conducted by Public/Private Ventures (P/PV) of Philadelphia, a private not-for-profit corporation that seeks effective approaches to job training and education for the disadvantaged. Funding for the demonstration is provided by Aetna Life and Casualty Foundation, The Ahmanson Foundation, The Edna McConnell Clark Foundation, The Ford Foundation, W. T. Grant Foundation, The William and Flora Hewlett Foundation, The Robert Wood Johnson Foundation, Lilly Endowment, James C. Penney Foundation, the U.S. Department of Health and Human Services and the U.S. Department of Labor. At the sites, day-to-day management and operation of the program are the responsibility of employment and training agencies, school districts and a number of other local institutions. Local program operation costs are covered primarily by Title II-B funds under the Job Training Partnership Act (JTPA) and local school district resources.

Three cohorts of disadvantaged youth have been participating in the demonstration in each of the five sites. The first cohort entered STEP in the summer of 1985. Cohort I participants received a summer of instruction and work experience in 1985, participated in STEP's school-year support (SYS) activities during the 1985-86 school year and returned for a second summer in 1986. The second cohort of youth entered the program in the summer of 1986, participated in SYS activities during the 1986-87 school year, and returned for a second summer in 1987. The third cohort entered the program in the summer of 1987 and are eligible to participate in school-year support during the 1987-88 school year and return for a second summer of services in 1988.

Previous reports have described implementation of the model during the first two summers of the demonstration and the short-term impacts of participation for the first two cohorts. This report focuses on new data collected during school year 1986-87, during the third summer (1987) and from follow-up interviews conducted during Fall 1987. Thus, the report covers the in-program experience of Cohorts II and III and some early postprogram experience of Cohort I. A detailed analysis of the costs of operating STEP is also presented.

For Cohort III youth, we examine impacts detectable by the end of the first summer of STEP participation. For Cohort II youth who participated in school-year support during SY 1986-87 and returned for their second summer of remediation, life skills and work experience, we examine mid-program school-year performance and end-of-program outcomes. For Cohort I youth, who completed STEP at the end of the 1986 summer, we examine a number of postprogram outcomes measured during the year following the end of the program enrollment.[1]

The remainder of this chapter provides background on the problems of dropping out of high school and of teen pregnancy, and addresses policy issues relevant to the demonstration.

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[1] The research design, hypotheses and analysis strategies are discussed more fully in Chapter III.

BACKGROUND

Every year, large numbers of youth lacking high school degrees and adequate literacy skills enter the labor market.[2] A recent assessment of literacy among young adults found that minorities are disproportionately represented among individuals with low levels of literacy, who show a tendency for dropping out (Kirsch and Jungeblut, 1986). This group's lack of basic skills impedes their chances of finding initial employment and limits their long-term employment and earnings prospects.

Increasing the educational attainment of youth at risk of dropping out of school will similarly increase their employment opportunities; this, in turn, will improve their ability to be economically self-sufficient. Educational attainment studies have shown better basic skill levels in reading and math to be related to employment and earnings (Jencks, et al., 1979; Berlin and Sum, 1988). In addition, numerous studies have shown that additional years of schooling result in additional income and that high school completion is a particularly important determinant of future income. The average high school dropout can expect lifetime earnings that will equal only two-thirds of the earnings of the average high school graduate (U.S. Department of Commerce, 1983).

The gaps between high school graduates and dropouts in earnings and employment are substantial and are growing steadily. Between 1965 and 1979, the disparity in the two groups' earnings grew by more than 20 percent (Sum, et al., 1983). During roughly the same period, the percentage of dropouts who were employed declined significantly--from 52.5 percent to 40 percent. The problem is especially acute among minority groups, particularly blacks and Hispanics. These groups are most strongly affected by poverty and are most likely to exhibit educational deficiencies that will limit their long-term chances to become economically independent and productive citizens.

The issue of the high school dropout rate and its implications will become even more important in the future. Demographic projections indicate that growing proportions of the youth population will be from the groups with the highest dropout rates: the economically disadvantaged and minorities. Using data from the March 1982 Current Population Survey, the National Center for Education Statistics (1985) estimated that among 20- to 24-year-olds, approximately 23 percent of blacks and 40 percent of Hispanics have not completed four years of high school. As the number of disadvantaged youth increases, the consequences of under-education and low levels of literacy will significantly affect society, as well as individuals.

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[2] The lack of a high school diploma and/or adequate basic skills, parenting responsibilities and other problems keep others from ever entering the labor market at all.

Over the next 10 to 15 years, the educational skills required by the marketplace are expected to be more demanding. A recent survey of employers in New York City found that only 18 percent of jobs were open to candidates without high school diplomas (Malizio and Whitney, 1984). Numerous entry-level jobs in the service sector are being generated, but fewer and fewer of them are being developed in industries or businesses that can provide career paths for unskilled workers. Even lower-level service positions, such as clerical workers, cashiers, and salesworkers, require basic skills in written and oral communication (National Academy of Sciences, 1984).

If high school completion rates and concomitant skill levels do not increase, "there will be a less literate pool of Americans from which colleges, universities, industry and the military will be able to draw to meet their human resource needs" (Kirsch and Jungeblut, 1986). Another prominent researcher (Levin, 1985) notes that a larger underprepared workforce will reduce economic competitiveness, increase the cost of providing public services, and produce a dual society with a large, poorly educated underclass. Kirsch and Jungeblut (1986) further predict that this scenario "will occur unless appropriate intervention strategies are developed and implemented to meet the diverse needs of current young adults, as well as to promote higher proficiencies among the younger, school-aged populations."

The STEP intervention focuses on two major predictors of dropout behavior. First, it targets youth who have had negative academic experiences, such as getting poor grades or being held back a grade level. Surveys most frequently cite such experience as the primary reason for youth dropping out of school. For example, approximately 42 percent of dropouts surveyed by the High School and Beyond study reported receiving primarily Ds in their classes (Institute for Educational Leadership, 1986). The portrait of the dropout that has emerged is one of a young person who is unable to function on a par with fellow students.

Remedial education programs have proved a promising way to remedy the academic problems of potential dropouts. Research on Job Corps participants and disadvantaged high school and college students has shown that well-designed remedial education programs are capable of producing modest but long-lasting academic gains (Taggart, 1981; Kapsis and Protash, 1983). And in a study of summer school students in Atlanta (Heyns, 1978), students gained, on average, the equivalent of one month for each month of instruction on the word knowledge portion of the Metropolitan Achievement Test (MAT).

A crucial determinant of academic performance seems to be the extent to which learning slows, stops or decays over the summer months. Findings from several studies indicate that this problem is particularly acute for disadvantaged youth, who consistently lose ground over the summer relative to their advantaged counterparts. A 1978 report to the New York State Board of Regents claimed that "educationally disadvantaged students lose about three to four months of their year's growth over the summer months, compared with students scoring above grade level, who showed one month's growth during the summer" (cited in Quinlan, et al., 1987).

Studies of compensatory education programs, which serve disadvantaged youth during the school year, invariably find that low-income youth lose more ground than more advantaged youth during the summer vacation when they are not in the program (Pelavin and David, 1977; David and Pelavin, 1978; Hammond and Frechtling, 1979). Hayes and Grether (1969) suggest that as much as 80 percent of the difference between advantaged and disadvantaged students may be due to differential summer learning. These findings indicate that summer learning is a critical factor in education (Heyns, 1987).

Although summer remedial programs are offered by many schools, most disadvantaged youth would rather spend their summer months working at a summer job. While work experience during high school pays off in future earnings and employment (Meyer and Wise, 1980), the effects are strongest for youth who graduate from high school (Berlin, 1983; Berlin and Sum, 1988; Taggart, 1981). Furthermore, results of the Youth Incentive Employment Program demonstration indicate that providing dropout-prone youth with jobs alone does not help keep them in school (Gueron, 1984).

The second predictor of dropout behavior that is addressed by STEP is adolescent parenthood. Although poor academic performance is the most commonly cited reason for leaving school, studies show that as many as three-quarters of female dropouts cite pregnancy and/or marriage as their reason for leaving school (Coombs and Cooley, 1968; Presser, 1978). A number of studies conclude that girls who become mothers "while they are in junior high school or high school complete on average fewer years of school, are less likely to earn a high school diploma, and are less likely to go on to college and graduate study than those who delay childbearing until their twenties" (Hayes, 1987). The consequences of adolescent parenting fall heavily on females, but research findings indicate that teen fathers are also more likely to drop out of high school prior to graduation than males who do not father a child (Hayes, 1987).

The relationship between teen parenting and educational attainment is a complex one, particularly among females. There is evidence that many adolescent mothers drop out prior to pregnancy and that becoming pregnant may be precipitated by poor academic performance. As Dryfoos (1985) points out, "if motherhood follows unsuccessful school experiences, it may be possible that upgrading the quality of education would have an impact on fertility rates."

A number of studies attest to the difficulty of a successful intervention once the first pregnancy has occurred--the most recent being a series of evaluations of Project Redirection (Quint and Riccio, 1985). Project Redirection researchers have concluded that strong support should be given to program efforts aimed at preventing the first pregnancy, in light of the complexity of the problems faced by young people once they become parents.

POLICY ISSUES

The STEP model is responsive to numerous aspects of the policy debate about education and employment of at-risk youth. The problems faced by disadvantaged youth are multiple and interrelated. STEP provides a model for addressing such issues as the need for integrated services at a reasonable cost, for early intervention and for an innovative approach to providing more schooling for youth who are not doing well in school. The four-year STEP demonstration tests the feasibility of implementing a multiple-service, low-cost intervention.

STEP operates within an already existing program that serves large numbers of disadvantaged youth, primarily through summer work experience--provided by SYETP. This program, authorized and funded under JTPA,[3] has several purposes: to "enhance the basic educational skills of youth; to encourage school completion or enrollment in supplemental or alternative school programs; and to provide eligible youth with exposure to the world of work" (JTPA, Part B, Sec. 251). Because STEP capitalizes on the resources of SYETP, it could potentially be implemented in a large number of jurisdictions for a relatively low cost.

Following an assessment of employment and training programs for in-school youth, the National Research Council's Committee on Youth Employment Programs recently recommended that SYETP be modified to address basic skills deficiencies among the disadvantaged youth it serves. Specifically, the panel made the following recommendation:

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[3] The Job Training Partnership Act (JTPA), enacted in 1982, was designed to provide training aimed at increasing the employment rate and earnings of participants. Congress viewed this training as a long-term investment in human capital.

> Attempts should be made to restructure some elements of the Summer Youth Employment Program [sic] to systematically test whether SYEP [sic] elements can be used to enhance basic education sufficiently to reduce school dropout rates. . . . Elements of SYEP [sic] could be structured so some skills training is added to the pure work experience in order to determine whether such training enhances the long-term employment effects of the program. (Betsey, et al., 1985:27)

In response to the growing concern regarding the basic skill levels of disadvantaged youth, Congress passed amendments to JTPA that directly address this issue. These amendments mandate that funds be spent on increasing literacy and reducing high school dropout rates. Although specific funding levels for remedial programs are not stipulated, the amendments require that service delivery areas (SDAs) assess the reading and math skills of all SYETP participants and that each SYETP program include a remedial education component, starting in 1987.

In response to a survey conducted by the General Accounting Office (GAO), SDAs reported a number of concerns related to the new amendment. First, many SDAs were uncertain as to how much a remediation program would cost. Given this undetermined cost increase and the concurrent reduction in federal funding for SYETP, SDAs expected to serve fewer youth and reduce the total number of work hours offered. Second, SDAs expressed concern about what type of remediation program would be effective and how to estimate the number of youth needing remediation (GAO, 1987). According to the survey, the majority of SDAs that provided remediation services in both 1986 and 1987 used the local school district as the remediation provider; instruction included lecture/discussion and individualized/self-paced formats and computer-assisted instruction. Slightly more than half of those SDAs offered academic credit for remediation and more than three-fourths paid the youth wages, bonuses, stipends or other payments for participation in remediation (GAO, 1987:3).

Year-round schooling is another proposed solution to the educational problems of at-risk youth. Year-round education, which generally means a reorganization of the school calendar rather than the provision of additional days of school,[4] has been promoted as a way to reduce learning losses that occur over summer vacation. With most year-round calendars, instructional blocks and vacation periods are shorter than in the traditional calendar and are distributed throughout the year.[5]

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[4] In a recent study of year-round education in California (where two-thirds of the nation's year-round programs are located), only one of the schools participating operated an extended program. The remaining programs all adhered to the traditional 180-day school year (Quinlan, et al., 1987).

Although year-round schooling has been proposed as a possible approach to improving educational achievement, most year-round programs are instituted to alleviate problems of overcrowding rather than directly improve the quality of education. When overcrowding problems cease, districts often revert to a traditional calendar regardless of whether educational achievement has increased. This reluctance to operate a year-round program is largely due to the inherent logistical problems.

Many of the problems associated with year-round education are related to community services and support. A year-round calendar requires alternative arrangements for child care and community recreational services. Employers of students are affected by a change in the school calendar, since the available summer work force is reduced. Another problem commonly faced by school districts involves parental complaints regarding the difficulty of coordinating their children's vacations. And major repairs and maintenance are difficult to schedule when buildings are in use throughout the year. Thus, in spite of any positive educational benefits associated with year-round education, the practical problems are often too great to overcome.

It should be further noted that while the California study provides some evidence that single-track, year-round schooling can be related to improved academic performance, year-round schools serving the most disadvantaged populations did not show improvement in achievement (Quinlan, et al., 1987:94-5). Thus, year-round schools may not be the best answer for addressing the needs of the most at-risk students.

Elements of the STEP model address a number of the issues relevant to these needs. Services are targeted toward those who stand to lose the most over the summer and are most in need of remedial instruction. Since youth are paid to participate, the opportunity to earn income through summer employment is afforded. Through participation in STEP, at-risk youth receive the benefits of instruction in math, reading and life skills that they need while they earn an income through a summer job.

The research component of the STEP demonstration addresses both the feasibility and effectiveness of providing services by building on the existing SYETP. The implementation and cost analyses presented in this report address many of the concerns expressed by SDAs regarding implementation of the new JTPA regulations. The impact analyses provide an indication of how participation in an enhanced and extended program benefits at-risk youth.

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[5] Of the year-round schools in California, 69 percent have a calendar with four 15-day vacations, 14 percent have two 43-day vacations and 11 percent have three 20-day vacations (Quinlan, et al., 1987).

ORGANIZATION OF THIS REPORT

The preceding discussion of STEP's background and the policy context of the demonstration provides a framework for the remainder of this report, which covers the third summer of the demonstration in detail. The STEP model, site descriptions and a summary of early demonstration findings are presented in Chapter II. The research design used in the evaluation of STEP is described in Chapter III.

Chapter IV presents an analysis of the implementation of STEP during the summer of 1987, with an emphasis on replication issues. The third cohort of STEP youth and those youth in Cohort II who returned for their second summer are profiled in Chapter V.

Impact analyses are discussed in Chapters VI, VII and VIII. Chapter VI focuses on impacts that can be detected over the course of one summer for Cohort III and the aggregate sample of the second and third cohorts; Chapter VII presents the estimated impact on mid-program school-year behavior and an analysis of outcomes that can be detected over the second summer of participation; Chapter VIII presents first-year postprogram outcomes for Cohort I.

An analysis of the costs to operate STEP is presented in Chapter IX. Chapter X summarizes the third year of the demonstration and discusses plans for Year Four.

II. THE STEP MODEL AND EARLY FINDINGS

The initial design of the STEP demonstration included a three-year operational phase during which two cohorts consisting of approximately 1,500 youth each would be enrolled--one in 1985 and the other in 1986. Youth were to be randomly assigned to either the treatment or control group. Treatment youth would receive 15 months of services--two summers of remediation, life skills instruction, and work experience, with school-year support (SYS) services provided during the intervening school year. In most sites, controls would receive one summer of work experience only. All youth were to be followed for five years, with the research phase ending in 1992.

While this basic program model was retained, P/PV instituted a number of programmatic changes as a result of experience gained during the first summer of operations. The most significant of these were the design and implementation of a new remediation curriculum, titled Practical Academics, and a strengthening of the SYS component.

Due to the success of these programmatic changes and the reduced severity or absence of start-up problems encountered during the first summer, the demonstration was expanded to include a third cohort of 1,500 youth, bringing the total research sample to 4,500. Cohort III entered the program in 1987. The operational phase of the demonstration will now conclude in 1988, with Cohort III treatment youth's second summer of services. The long-term research phase of the demonstration, which includes the collection of school records data and data from personal interviews with Cohort III youth, has also been extended and will now be completed in 1993.

TARGET POPULATION

STEP targets disadvantaged youth who are "at risk" of dropping out of school and becoming adolescent parents. Eligibility is largely determined by SYETP, which pays participants' wages. To meet federal SYETP eligibility requirements, STEP youth must be at least 14 years old and economically disadvantaged. However, STEP further requires that youth be 14 or 15 years old, and be educationally deficient but still attending school.

STEP is directed at 14- and 15-year-olds rather than older youth for several reasons. First, although these youth are interested in obtaining summer jobs, opportunities for them to do so are limited. Second, most are making the difficult transition from junior to senior high school, a point at which problems often develop. The dropout rate begins to accelerate as youth both enter high school and approach age 16--the age at which they can "legally" drop out of school in most states. Third, students are thought to be more receptive to remedial efforts at the ages of 14 and 15 than they are at later ages. Finally, as adolescents, these youth are frequently becoming sexually active and are therefore running the risk of becoming parents. Thus, their need for information on sexual attitudes and behavior is heightened.

Since substandard academic performance is a primary reason for youth leaving school without graduating, STEP also concentrates on an academically at-risk population. Eligible youth are those who perform below their grade level in reading or math, according to recent standardized test scores and/or a recent history of grade retention. However, a fourth-grade reading floor is imposed to ensure that youth have the minimal decoding skills needed to benefit from the remediation curriculum.

PROGRAM COMPONENTS

With some variation in implementation across the five demonstration sites, each program focuses on four key components: remediation, life skills and opportunities, work experience and school-year support. Implementation of these components is described in detail in Chapter IV; they are reviewed briefly here.

During each of two summers, participants are offered approximately 90 hours of remedial instruction in basic reading and math skills, 18 hours of Life Skills and Opportunities instruction, and at least 80 hours of work experience. During the intervening school year, SYS activities designed to encourage youth to remain in school are offered. These activities are aimed at helping STEP participants maintain the progress made in the first summer and encouraging them to return for the second summer.

The remediation component provides a minimum of 90 hours of skill-based group and individually paced instruction. The curriculum, Practical Academics, was designed to address two major interlocking problems--deficiencies in basic skills, and a lack of self-confidence and sense of control over the learning process. The remediation plan requires that students spend roughly 20 percent of their instructional time in computer-assisted instruction (CAI), which is intended to motivate youth and promote faster learning. In addition, at least 20 minutes each day is spent in silent sustained reading.

Practical Academics is built around a set of learning "modules" that offer important advantages for youth who are performing poorly in school. P/PV staff and a team of curriculum experts developed the remediation modules[6] to address the needs of the target population and produce essentially uniform programs at all sites. Each module uses topics that are expected to be of interest to STEP's student population while addressing specific reading and math skills, including categorizing and sequencing written information, distinguishing fact from opinion, interpreting charts and graphs, and solving math word problems.

The modules present a process for teaching skills through modeled instruction and applied, individualized follow-up activities. Each skill is reinforced at increasing levels of difficulty and students can work up to a level that challenges them. Meanwhile, they receive prompt feedback on skill attainment and progress. CAI and silent sustained reading constitute the remainder of the remediation schedule. This individualized approach was adopted to maximize students' "time on task" by allowing them to work with materials on appropriate skill levels.

Although all modules are complete, they are designed to accommodate modification or enhancement to meet local needs. In addition to the modules developed by P/PV, several sites have developed modules according to P/PV guidelines that have been approved for local use.

The Life Skills and Opportunities (LSO) component of STEP stresses responsible social and sexual attitudes and behavior. Two volumes of LSO curriculum (one for each summer) cover personal and sexual decision-making, job-equality issues, the consequences of teen pregnancy, AIDS, substance abuse, ways to avoid pregnancy (including abstinence) and sources of family planning services. A constant focus is the relationship between decisions related to sexual behavior and youth's personal development. For example, the economic, health and social implications of early childbearing are fully explored. In addition to enhancing students' understanding of the consequences of parenting, the LSO curriculum discusses the benefits of delaying sexual involvement and explains the consistent and effective use of contraception.

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[6] P/PV developed nine thematic modules for 1985: two job skills modules--"Adjusting to the Working World," and "Getting and Handling the Job"; three school-based modules--"Pioneers in Their Field," "Early Explorers" and "World Cultures: Pros and Cons of Arranged Marriages"; and four life skills modules--"Substance Abuse," "Getting the Message from the Media," "Decisions" and "Self-Identity."

In 1986, P/PV developed five additional modules: "Legal Rights," "Newspapers," "Self-Identity: Roles," "Substance Abuse" and "Jobs: About Work." P/PV also substantially revised two modules: "Adjusting to the Working World" was retitled "Job Skills: Adjusting to the World of Work," and the original "Self-Identity" module became "Self-Identity: Events."

P/PV, in conjunction with outside consultants, designed the curriculum to meet the special needs and circumstances of the STEP target population, many of whom are minority group members and many of whom are already sexually experienced. During their first summer, students participate in approximately 18 hours of LSO instruction. Lectures, discussions, films, role-plays, field trips and outside speakers are used to stress for youth the importance of planning for the future. These activities are intended to help youth consider goals, responsibility, the decision on whether to have sex and how to have sex responsibly. Volume II of the curriculum, used in the second summer of the program, provides an additional 18 hours of instruction that reinforce information covered in the first summer and introduces new material on substance abuse, general health issues and career planning.

In the work experience component of STEP, youth are usually assigned to jobs either in their neighborhood or near their remediation site in order to minimize transportation problems. STEP youth are younger than the majority of SYETP participants; thus, their inexperience and child labor law requirements limit the types of jobs available to them. Most STEP youth are placed in maintenance, recreation, clerical and child care aide positions. A few are assigned to special group projects, such as producing a newspaper or painting a mural. Sites attempt to give youth "better" jobs during the second summer. All are paid minimum wage for their participation in both work experience and remediation/life skills instruction.

The school-year support (SYS) component spans the period between participation in the first and second summers of STEP. This support involves periodic group activities and individual contact between youth and a designated counselor/advocate. Counselors/advocates aim to help youth remain in school by contacting them a minimum of twice a month and either providing or referring them to needed tutorial and/or counseling services. They also encourage youth to return for a second summer in STEP. The SYS component was intensified beginning with the 1986-87 school year. Each site now has an SYS coordinator who is responsible for planning and managing SYS activities. Coordinators are responsible for hiring, training and supervising counselors/advocates, and for planning group activities. Counselors/advocates are responsible for making individual contacts with youth, linking them to social services as needed, and monitoring their school attendance and performance.

Parental involvement in SYS is encouraged through periodic informational mailings and meetings held at the beginning of the school year. Parents are encouraged to assist in organizing and conducting group activities, and all parents formally approve the participation of their child in STEP.

SITE DESCRIPTIONS

Site selection for P/PV's STEP demonstration was based primarily on the sites' management and administrative experience and capacity; the cooperation they had secured of relevant institutions, notably the JTPA agency and the school district; the extent of local resource commitment; their ability to serve adequate numbers of youth, with a racial/ethnic mix that would make the results useful to other jurisdictions; and their willingness to collect the data and carry out other requirements of the research design. Following a careful review and screening process, P/PV selected five sites: Boston, Massachusetts; Fresno, California; Portland, Oregon; San Diego, California; and Seattle, Washington. P/PV then hired on-site coordinators to serve as liaisons between P/PV and site operators, monitor the program, collect data and provide technical assistance to the local agencies who operate the program.

The following sections briefly describe the five sites' management structure[7] and summarize results for the first two summers of STEP implementation.

Boston

In 1987, oversight of STEP in Boston was the responsibility of the Mayor's Office of Jobs and Community Services. That office contracted with Action for Boston Community Development (ABCD), a community-based organization, to operate both SYETP and STEP.

Remediation and life skills classes were held at four local universities: Boston University, University of Massachusetts/Boston, Northeastern University and, beginning in 1987, Harvard University. Treatment youth were assigned to attend classes at the university nearest to their home. They attended remediation classes Monday through Friday mornings, participated in life skills classes for 90 minutes two afternoons per week, and worked on campus three afternoons per week. LSO instruction was provided by ABCD counselors, who also monitored work experience. Control youth worked 25 hours per week at regular SYETP jobs, most of which were sited in their home neighborhood.

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[7] See Chapter IV for a detailed discussion of each site and Table IV.1 for a depiction of the agencies involved and their responsibilities for operating STEP.

Fresno

As the lead agency, the Fresno Private Industry Council (PIC) was responsible for operational and fiscal management of STEP in Fresno. The PIC contracted with the Economic Opportunity Commission for outreach, recruitment, intake, economic eligibility determination and worksite development for STEP youth. The remediation and life skills components were the responsibility of the Fresno Unified School District.

Remediation and life skills classes were held at one middle school five mornings a week. Treatment youth worked from three to four hours each afternoon. Control youth worked from seven and a half to eight hours per day. Both groups were assigned to jobs at schools, parks, government offices and private businesses throughout the city.

Portland

As in Fresno, the PIC in Portland was responsible for all aspects of STEP. Staff hired by the PIC to operate STEP were responsible for recruitment, eligibility determination, intake, worksite development, job placement, and monitoring and managing of the various components of STEP. Although teachers and aides were hired jointly by the PIC and Portland Public Schools (PPS), they were paid by PPS. A PPS building administrator was responsible for remediation and life skills activities, but day-to-day supervision was the responsibility of the PIC's STEP coordinator.

STEP treatment youth attended remediation and life skills classes each morning at an area high school. These youth worked approximately three hours each afternoon, while control youth worked six to seven hours a day. Approximately 25 percent of the jobs were custodial positions with PPS, but youth also worked at hospitals, parks, museums and government offices.

San Diego

The San Diego Regional Employment and Training Consortium was the lead agency with primary responsibility for STEP in San Diego. The remediation and life skills components were subcontracted to the San Diego City Schools. The work experience component was subcontracted to the San Diego Regional Youth Employment Program.

The number of sites used for remediation and life skills classes was reduced in 1987 from five schools to two. Classes were held every morning, and treatment youth worked three hours each afternoon. Control youth generally worked seven hours four days per week. As in the previous summer, San Diego offered control youth an opportunity to return for a second summer of work experience. Most treatment and control youth were placed in jobs near their home, primarily in custodial, recreation, child care, and clerical positions.

Seattle

Implementation of STEP in Seattle was the responsibility of the city's Department of Human Resources (DHR). DHR had responsibility for all aspects of STEP, but received financial support from the Seattle/King County PIC, Seattle Public Schools (SPS), the Washington State Commission for Vocational Education, and United Way of King County.

A centrally located middle school served as the site for remediation and life skills classes. For the first time, remediation and life skills classes in Seattle were offered only in the morning in 1987. In addition, control youth in Cohort II were offered an opportunity to return for a second summer of work experience through SPS's Work Training Program. Control youth worked seven hours each day. Jobs for returning youth included individual assignments in clerical, maintenance and child care positions. Cohort II youth were assigned to group projects in grounds maintenance, neighborhood clean-up and theatrical programs.

EARLY PROGRAM FINDINGS

The remainder of this chapter summarizes the implementation of STEP in the first two summers of the demonstration and reviews the previously reported findings of the short-term impact analyses.

The 1985 Summer Experience

Positive findings from the pilot study led P/PV to expand STEP research to a three-year national demonstration, beginning in the summer of 1985. The five participating sites each recruited approximately 330 youth, who were randomly assigned to either a treatment or control group.

The implementation analysis from the first summer further confirmed the operational feasibility of STEP. The first cohort of STEP participants were racially diverse, multiply disadvantaged and clearly in need of some type of intervention. Research showed that STEP was successful in producing a number of short-term benefits for participants in both educational and fertility-related outcomes.

In its first summer, STEP was successful in mitigating the substantial learning losses that occur over the summer in the absence of treatment. Treatment youth outscored their control group counterparts in both reading and math by approximately one-quarter of a grade equivalent. However, the program was not powerful enough to completely eliminate summer learning losses for treatment group members. Treatment group test scores in both reading and math were lower at the end of the summer than they were at the beginning of the summer. Control group learning losses, however, were significantly more substantial than those experienced by treatment youth.

Participation in STEP also had a substantial impact on participants' knowledge of contraceptive information. Knowledge of birth control methods and availability significantly increased for every site, racial/ethnic and gender subgroup.

The 1986 Summer Experience

While generally pleased with the implementation of the model in 1985 and the research findings for the first cohort of STEP participants, P/PV significantly redesigned and strengthened the remediation and SYS components following the first year of operations.

During the first summer, the remediation curriculum framework (CORE) provided guidelines and detailed information for sites to use in developing local curricula. Sites found, however, that they had neither the time nor the expertise to develop a new curriculum for STEP teachers at the local level. Thus, P/PV developed the scripts for topic-based learning modules that would be the foundation for the local remediation programs.

P/PV's Practical Academics retains CORE's basic instructional approach of a skill-based, individually paced curriculum, but its learning modules put basic skills instruction in the context of student-centered issues. Modules focus on critical thinking skills and include applied learning activities. Sites select supplementary skill reinforcement, CAI materials and books for silent sustained reading. While sites are also encouraged to modify and enhance the curriculum modules for local needs, the introduction of Practical Academics has greatly reduced the responsibility of local developers. To further strengthen the planning and implementation of the remediation component of STEP, each site was required to hire a lead teacher who was responsible for the overall management of remediation, beginning with curriculum development in the spring and continuing through the summer program.

The SYS component was also significantly changed following the first year of operation. During the 1985-86 school year, four of the five sites operated an SYS program. However, the programs provided few real services and youth attendance at group events was poor. In response to the disappointing results of the first year, P/PV made structural and management changes for the 1986-87 program and intensified its oversight of SYS.

In addition to changes made by P/PV following the first year of STEP operations, a number of improvements were noted during the second summer that resulted from a decrease in the severity of problems associated with program start-up. STEP requires the collaboration of a number of agencies that do not have much experience with joint ventures, including PICs, school districts, and employment and training agencies. After a year of experience, P/PV observed stronger collaboration among these agencies. The enhanced cooperation led to improvements in planning and increased the operational efficiency of the program. More specifically, the year's experience resulted in improvements in the identification and recruitment of students, teacher selection and training, and the implementation of selected portions of program components, such as CAI.

Although many of the demonstration's start-up problems were reduced or eliminated in the second summer, the introduction of a new curriculum created other problems. Teachers needed more training and guidance in using the learning modules, which were an unfamiliar instructional format, since it was important that instructional emphasis remain on academic skills (i.e., reading and math) rather than on module content topics. Integrating the CAI exercises, skill reinforcement activities and materials for silent sustained reading into skills instruction dictated by the modules also posed some problems for sites.

STEP's impact on basic skills for Cohort II was more than twice as strong and more widespread than the effects observed for Cohort I, with significant impacts in every site and all racial/ethnic and gender subgroups. Most of the summer learning loss in reading experienced by control youth was stemmed for participants, with treatment youth outscoring control youth by six-tenths of a grade equivalent. Also, treatment youth improved their math skills over the summer, scoring eight-tenths of a grade equivalent higher than control youth.

Continuing the impressive life skills-related results observed for the first cohort in 1985, all subgroups of Cohort II treatment youth improved their knowledge of contraceptive methods and availability. This improved knowledge may have been translated into behavioral changes. Sexually active STEP participants were 53 percent more likely to use contraception than their control counterparts at the conclusion of the summer program.

The substantially improved performance of the second cohort posed interesting questions. Were the 1986 results due to the introduction of the stronger, more prescriptive curriculum and the increased operating experience of the sites or were they a statistical aberration? To better test the strengthened STEP model, P/PV decided to add a third cohort of youth and extend the demonstration an additional year. The third cohort also provided an opportunity to implement the intensified SYS component for a second time during the 1987-88 school year.

The third cohort added more than 1,500 youth to the research sample, bringing the total to more than 4,500 (evenly divided between treatment and control groups). Given the substantial improvements, both operationally and in the structure of the remediation and SYS components, that were made following the first year of operation, we feel that the experience of Cohorts II and III represent the "best test" of the STEP model. Consequently, the research presented here focuses on the second and third cohorts.[8] Results are presented for the new cohort, Cohort III, and for the combined sample of Cohorts II and III.

Before discussing the impacts of STEP in Year Three, however, we will summarize the demonstration's research design in Chapter III, describe the implementation of the program in Year Three in Chapter IV, and describe the STEP participants in Chapter V.

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[8] An analysis of aggregate test score data from all three cohorts was performed, however, and will be presented in Chapter VI. It is important to note that this analysis represents an average of the impacts detected for each cohort. Since tests for statistical significance are sensitive to sample size, these aggregate findings may result in more statistically significant outcomes without a concomitant increase in substantively important results.

III. RESEARCH DESIGN

The evaluation of STEP is designed to assess both the impact of the program on participants and the feasibility of implementing the model in various settings and on a large scale. Research began with the pilot program in 1984 and will continue until 1993--five years after completion of the four-year operational phase of the demonstration.

The major components of the research are an implementation analysis and an impact analysis. The implementation analysis assesses the viability and replicability of STEP by describing the processes involved in planning, coordinating and operating the program--including problems encountered and their solutions. The impact analysis examines the impact of STEP on academic performance, development of responsible social and sexual attitudes and behavior, reduction of adolescent parenthood, school retention and high school graduation, and early labor force experience.

IMPLEMENTATION ANALYSIS

The implementation analysis examines factors that contribute to understanding the demonstration's achievements and replicating the STEP model. Topics related to initial implementation of the demonstration, such as developing and coordinating institutional arrangements, were reported previously (Branch, et al., 1986). While this report focuses primarily on developments related to the STEP program in the summer of 1987, it also reviews STEP implementation over the life of the demonstration, highlighting problems encountered, attempted solutions (whether successful or unsuccessful) and adjustments of the model. Although some implementation problems are specifically related to the demonstration and/or the research requirements, many will be faced by any program operator attempting to institute a STEP program. Thus, the implementation discussion should prove useful to all operators. Some of the key issues to be addressed include:

- Differences in operational and management strategies across sites, including an assessment of which seem to work best;
- Problems encountered and solutions attempted with respect to reenrolling Cohort II, recruiting Cohort III and operating the program with a double cohort;
- Responses to key features of the program by lead teachers, participants and instructors;

42

- Characteristics of program participants--both the first-year Cohort III participants and the returning Cohort II treatment and control youth; and

- An analysis of program costs--i.e., the incremental costs of operating STEP above and beyond the cost of offering summer work experience to control youth.

The implementation analysis relies on both qualitative and quantitative data. Qualitative data were collected primarily by P/PV's local site coordinators, who were involved in program activities associated with the initial development and implementation of STEP as well as in planning changes made prior to the second and third summers.

These site coordinators are well-positioned to pinpoint aspects of the program components that appear to be particularly effective, as well as those that are problematic. Throughout the summer, they spent many hours observing remediation and life skills classes. They also visited and observed a sample of both treatment and control worksites and conducted interviews with job development and placement personnel. At the conclusion of the summer, site coordinators prepared case studies that summarize the implementation of the model at each site. Their assessments are supplemented by observations made during periodic site visits by P/PV's central research and program staff and by expert life skills and remediation consultants.

Quantitative data were obtained from application forms, answers to questionnaire items designed to ascertain participants' responses to key program elements, payroll records and STEP budget and expenditure information. In addition, P/PV's research staff conducted semistructured interviews with lead teachers in all five sites during the summer. Remediation, CAI and life skills instructors completed standardized questionnaires concerning their background and experience and their perceptions of STEP.

IMPACT ANALYSIS

The impact analysis assesses the effect of STEP participation on educational, fertility-related and work-related outcomes. Program impacts are determined by comparison of outcomes for treatment youth with those for the randomly assigned control group who participated in work experience only through SYETP. The ultimate concerns of STEP are high school graduation rates, teen parenting and youth employment; however, outcomes in related areas are also observed and analyzed--at multiple points in time.

After one summer of STEP, we examine whether participants exhibit the following outcomes in relation to the control youth:

- A greater retention or improvement of their reading and math skills;
- Higher expectations with respect to educational attainment;
- A greater knowledge of effective contraceptive practices and the consequences of adolescent pregnancy; and
- A greater tendency toward reduced levels of sexual activity and/or improved contraceptive use.

Beginning with the school year following the first summer of STEP participation, we examine whether treatment youth:

- Exhibit improved attendance and academic performance during the school year; and
- Experience a reduction in year-to-year school dropout rates.

Over the long term, we examine whether STEP treatment youth:

- Experience an increase in high school graduation rates;
- Experience a reduction in adolescent pregnancy and parenthood; and
- Achieve increased employment and earnings in their early labor force experience.

Impacts presented in this report include the effects of one summer of program participation on youth in Cohort III (those who entered the program in 1987); we also combine these results with the first-summer results for Cohort II. Impacts on outcomes that occur during the mid-program school year and following the second summer of treatment are assessed for Cohort II youth. Finally, longer-term impacts for Cohort I are presented, including post-program impacts on schooling, fertility and early performance in the labor force.

DESIGN STRATEGY

To examine the issues described above, STEP employs an experimental design involving random assignment of eligible youth to treatment and control groups, following uniform guidelines provided by P/PV. After determining youth's program eligibility and obtaining parental consent,[9] P/PV's site coordinators randomly assigned half the eligible youth at each site to the STEP treatment group, which divided its time between classes and work experience. The remaining youth were assigned to the control group and spent all program hours in work experience provided by the federal summer jobs program. Both treatment and control youth were compensated at minimum wage for all program hours, whether work experience or instructional time.

Random assignment is critical to the research design and the determination of program impacts. Because eligible youth are randomly divided between the treatment and control groups, both groups are statistically equivalent on all observed and unobserved characteristics at baseline. Thus, the experience of the control group is indicative of the experience treatment group members would have had in the absence of STEP. Therefore, we can be fairly confident that differences in outcomes that arise between the groups are attributable to the program and not to other factors.

The initial design of the demonstration called for a combined sample of 3,000 treatment and control youth to test STEP's major impact hypotheses. A sample of this size enables us to be reasonably certain we will detect policy-relevant impacts, given reasonable assumptions regarding the explanatory power of the variables in the multivariate analysis and sample attrition over the course of the follow-up period. The original sample of 3,000 was built up over a two-year period, with 300 youth enrolling each year (1985 and 1986) in each of the five sites. Due to the success of the second-summer program operation, the demonstration was expanded to serve a third cohort of youth who entered in the summer of 1987, increasing the total sample to approximately 4,500 youth.[10]

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[9] Before youth were accepted, written parental consent was obtained for them to participate in a research study involving random assignment and to participate in remediation and life skills components if assigned to the treatment group. Parental consent was also needed for schools to release youth's records. While youth were informed of their selection for STEP and a summer job as soon as the eligibility determination process was complete, the specific assignment to treatment or control status was withheld until completion of the pretest.

[10] However, as discussed in Chapter II, the analysis reported here and the follow-up research to be conducted through 1993 will focus primarily on the aggregation of data from the second and third cohorts.

DATA COLLECTION STRATEGY

Quantitative data on the individuals in the STEP sample have been and will continue to be assembled from a number of different sources and will be obtained from youth at various points throughout the demonstration (see Table III.1). Among the sources of data are participants' performance on standardized tests, responses to self-administered questionnaires, program records, school records, and questionnaires completed by remediation and life skills instructors. Beyond the demonstration phase, data are being collected through annual school records and telephone or in-person interviews conducted with both treatment and control youth.

Program Records

Program records provide important information about participants and their program experiences. The most prominent are application forms, which verify demographic information and economic eligibility for the federal summer jobs program, and program timesheets, which provide a record of hours spent in classes and at the worksite. Program records are also a major source of information about the costs of operating STEP.

Academic Testing Over the Summer

The reading and math subtests of the Intermediate Level Metropolitan Achievement Test (MAT) Survey Battery provide the primary measures of academic performance over the summer. These subtests, consisting of 60 items in reading comprehension and 50 items in math, provide nationally normed performance measures. The intermediate-level test is most appropriate for students performing at the fifth- to seventh-grade level. The intermediate level was selected based on the finding that STEP youth in the pilot study generally attained scores in this range at baseline.

Once random assignment is completed, both treatment and control youth take the MAT to assess baseline reading and math abilities.[11] Youth are retested at the end of the first summer. To protect against memory effects, participants are administered one form of the MAT as a pretest and another as a posttest. The two forms--Forms JS and KS--are judged to be functionally equivalent; in other words, the score attained is independent of the form being administered.

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[11] Although random assignment occurred before testing, neither proctors nor students were informed of the assignments until after testing.

Table III.1

SUMMER TRAINING AND EDUCATION PROGRAM
DATA BASE BY SOURCES

VARIABLES	DATA SOURCE
Demographic Characteristics	
Age	Application Forms School Records Baseline Questionnaire
Sex	
Race/Ethnicity	
Household Composition	
AFDC Receipt	
Single-Parent Household	
Marital Status	
Public Assistance Recipient	
Spanish Surname	
Language Spoken at Home	
Academic Performance	
Reading	Metropolitan Achievement Test (MAT), Survey Battery, Intermediate Level I, Forms JS and KS
Mathematics	
Education	
Highest Grade Completed	Application Forms School Records Baseline Questionnaire Follow-up Interviews
Ever Repeated a Grade	
Grade Repeated	
Number of Days Absent in Previous Year	
School of Attendance at Intake	
Enrolled or not	
Transferred from Previous Year's School	
School Transferred to	
Attendance (excused and unexcused absences)	
Grade Point Average	
Grade Retention/Progression	
Date of Graduation	
Date Officially Withdrawn	
Reason for Withdrawal	
Education Aspirations	
Standardized Test Scores	

Table III.1 - (cont'd)

SUMMER TRAINING AND EDUCATION PROGRAM
DATA BASE BY SOURCES

VARIABLES	DATA SOURCE
Fertility/Family Planning	
Percent Sexually Active	Baseline Questionnaire
Age at First Intercourse	Endline Quesionnaire
Contraceptive Knowledge	Follow-up Interviews
Contraceptive Utilization	
Knowledge of Consequences of Adolescent Pregnancy	
Number of Pregnancies	
Age at First Pregnancy	
Outcomes of Pregnancies	
Number of Children Fathered	
Numbers of Births	
Date of Birth for Each Child	
Child Care Assistance	
Employment	
Previous Employment	Baseline Questionnaire
Career Aspirations	Endline Questionnaire
Attitudes Toward the Work-Roles of Men and Women	Follow-up Interviews
Employment Status and History	
Hours per Week	
Type of Occupation	
Earnings	
Welfare Assistance	
Unemployment Compensation	

Although the initial research design called for testing both treatment and control youth in the second summer, the pilot study experience resulted in revision of this plan. The experience indicated that without the offer of employment during the second summer, few control youth returned for testing. The expense and effort of locating, recruiting and testing controls on a demonstration-wide basis could not be justified. In 1987, San Diego and Seattle decided to offer control youth a second summer of work experience and both groups were tested in the second summer in these sites; test score and questionnaire data were not collected during the second summer for controls in other sites.[12]

To minimize the possibility that control youth would have less motivation to perform at the posttest than treatment youth--and would thereby spuriously elevate the estimate of program impact--an incentive scheme was devised to provide both groups with an external reason for doing their best. Any youth taking the MAT received one chance in a drawing for one of 15 $50 prizes awarded at each site. An additional chance was received for each correct answer. Thus, a youth could maximize his/her chances of winning by correctly answering as many questions as possible, while youth responding at random, drawing designs on the answer sheet or simply not answering the questions would decrease their chances of winning. This incentive was available to both treatment and control youth at both the pretest and posttest. Assuming that these incentives had their predicted effects, any control group losses observed were the result of true losses in academic skills.

In addition to this regulation of students' motivation to perform well on the tests, care was taken to ensure that testing conditions were identical for treatment and control youth. Sites were required to use the same locations and proctors for treatment and control youth. In most cases, treatment and control youth were tested side by side in the same classrooms.

Questionnaires

Self-administered questionnaires completed by youth in conjunction with testing at the beginning and end of the first and second summers provided data on demographic characteristics and on measures of attitudes, knowledge and behavior that are relevant to the program. Demographic data collected include household composition, languages spoken, grade enrollment, grade retention and absenteeism. Attitudes and knowledge measured include self-esteem, educational expectations, occupational/

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[12] Although questionnaire and test data were not collected for most control youth in the second summer, school records and youth interview data will be collected for all treatment and control youth during the longitudinal phase of the study.

career aspirations, awareness of the consequences of adolescent pregnancy and birth control knowledge. Responses to various components of STEP were measured using the endline questionnaires.

Teacher Questionnaires

To supplement the data collected from youth, STEP instructors completed questionnaires at the conclusion of both the 1986 and 1987 summer programs. Teachers were asked to provide basic demographic information on themselves, such as their race/ethnicity, gender and educational background. The questionnaires also obtained data on the teachers' experience with the population served by STEP and their experience with individualized teaching methods, as well as their general response to the program. These data were merged with the individual student records so that any differential impacts due to teacher characteristics could be detected.

School Records

Data from school records are essential in determining the long-term effects of program participation on school performance, retention and high school graduation. School districts in the participating demonstration sites have agreed to provide school records data for the duration of the research phase of the project, including standardized test scores, attendance, grade progression, and dates of withdrawal, transfer and/or graduation for each youth in the STEP sample. In addition, the schools have agreed to provide data on local policies and procedures that will facilitate interpretation of student records. They will also lend assistance in tracing and gaining access to the records of students who transfer to other school districts.

Collection of school data began at the end of the 1985-86 school year for Cohort I treatment and control youth; data for SY 1986-87 were collected for Cohorts I and II. The school districts provided information on each youth still in its domain. For students who transfer out of their school district, either before or during the school year, the old school provides P/PV with the name and address of the student's new school (to the extent possible). P/PV then contacts the new school to obtain the required information. If the original school does not know where the youth is, P/PV tries to locate the youth by calling contact people, such as parents or other relatives. Through such tracking methods, we are able to collect the necessary data and identify the school enrollment status for a high percentage of youth. P/PV will continue to track all students in the STEP sample and collect school data through 1992.

Follow-up Interviews

After the operational phase of the demonstration, follow-up information on STEP youth from both the treatment and control groups in Cohorts II and III is collected through a minimum of three telephone or in-person interviews.[13] The first wave of follow-up interviews were conducted with youth in Cohorts I and II between October 1987 and January 1988. P/PV contracted with Abt Associates, Inc. to conduct telephone interviews with STEP youth. In-person interviews were conducted with youth who could not be reached by telephone. The data collected through these interviews address issues of the long-term impact of STEP participation that cannot be determined from school records data. Interviewers gather retrospective information on sexual activity and contraceptive use, pregnancies, children fathered, pregnancy outcomes, employment and earnings history, current employment status and earnings. They also verify educational data, including current enrollment status, grade progression, educational expectations, high school graduation and post-high school education.

Summary

Data are being collected during both the operational phase of the program and the longitudinal follow-up study of STEP youth. During the operational phase, the primary data sources are the summer tests, questionnaires and program records. All youth are scheduled to attend testing sessions at the beginning and end of the first summer of STEP. Treatment youth who return for a second summer and control youth who accept second-summer jobs attend additional testing sessions at the beginning and end of the second summer.

Longitudinal data collection began with the acquisition of school records data on Cohort I for the 1985-86 school year. School data for all treatment and control youth will be collected annually through 1992. Follow-up interviews began in Fall 1987, when treatment and control youth in the first two cohorts were contacted. Beginning in Fall 1988, youth in Cohorts II and III will be interviewed every two years through 1992.

The data available to analyze for this report include baseline and endline first-summer test and questionnaire data for all three cohorts, beginning and end of second-summer test and questionnaire data for returning Cohort I and II treatment youth (and for control youth in San Diego and Seattle), school records data for Cohorts I and II and the first wave of interview data for Cohorts I and II.

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[13] Current plans call for one follow-up interview with youth in Cohort I, conducted in Fall 1987. Cohort II youth were also interviewed at that time. Subsequent interviews will be held during the fall in 1988, 1990 and 1992 for all youth in Cohorts II and III.

ANALYTIC STRATEGIES

Estimation of the impact of participation in STEP relies heavily on multivariate analysis. Randomization ensures that the treatment and control groups are similar with respect to both observable and unobservable characteristics at the point of randomization. Since differences that arise in the future can therefore be attributed to the program, simple comparisons of mean outcome measures for the original treatment and control groups provide unbiased estimates of the program impacts. If, however, outcome measures are not available for all members of the original treatment and control groups because of non-response or missing data, simple comparisons can be misleading. Regression analysis provides more precise estimates of program impacts by controlling for differences in observable (baseline and demographic) characteristics between the two groups. By controlling for the variation due to baseline variables, the remaining unexplained differences in mean outcome measures can be attributed to the program with greater precision.

A similar specification is applied to most of the regression models used to analyze the impact of STEP participation. The explanatory variables include: basic demographic characteristics, such as gender and race/ethnicity; the demonstration site; variables that describe the youth's home environment, such as family size and birth order; and educational characteristics, such as absenteeism and educational expectations.

Learning and school data analysis models contain additional education-related characteristics, including youth's grade level at program entry, whether the youth have ever been retained in a grade, their enjoyment of reading and math, and their baseline scaled scores in reading and math MATs.[14] Data that describe characteristics of the STEP teachers for the 1986 and 1987 programs were also collected and are included in the learning analysis for the second and third cohorts.

Models used to estimate the impact of STEP on contraceptive knowledge and use, consequences of adolescent pregnancy, delay of sexual initiation, contraceptive use, and pregnancies and children fathered include baseline measures of these fertility-related outcome variables in addition to background and demographic variables.

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[14] The baseline reading score is included in both the reading and math equation, while the baseline math score is included only in the math equation. All school data outcome analyses include both baseline reading and math scores.

In general, the multivariate models used to estimate the impact of STEP on various outcome measures take the following form:

$$(1) \quad Y_2 = a + b_1 Y_1 + b_2 X + b_3 T + e_1$$

where:

Y_2 = the postprogram (endline) value of the variable of interest

Y_1 = the preprogram (baseline) value of the variable of interest

X = a vector of explanatory variables

T = whether the youth received STEP treatment

a, b_i = coefficients

e_1 = a stochastic disturbance term with a mean of zero and a constant variance

This specification makes it possible to control for preexisting differences among youth while the impact of STEP is estimated (b_3).[15] However, if the treatment and control groups differ with respect to unobservable characteristics, such as motivation, estimates from these regressions would be biased. Before conducting analyses, the nature of sample attrition is explored. If a differential percentage of treatment and control youth are found in the analysis sample, the statistical sample-selection correction factor, first developed by Heckman (1979), is included in the regression.[16]

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[15] This model is a more robust specification than one that analyzes change scores. An analysis of change scores assumes that the amount of change and pretest level of the outcome measure are perfectly related. If that assumption is violated, an analysis of change scores is a misspecification of the model and the resulting estimates of the coefficients are incorrect. The model estimated for the analysis reported here controls for pretest level if this assumption is violated and is equivalent to the change score model if this assumption holds.

The overall impact of STEP is estimated by the dichotomous variable T, indicating control group status. In addition to estimating the overall effect of the program using equation (1), a series of subgroup-treatment interaction variables are used to estimate the effect of the program in specific sites and to estimate the effect of STEP on racial/ethnic and gender subgroups. Algebraically, equation (1) is modified as follows:

$$(2) \quad Y_2 = a + b_1 Y_1 + b_2 X + b_3 T + c_1 TS_1 + \dots + c_4 TS_4 + e_2$$

$$(3) \quad Y_2 = a + b_1 Y_1 + b_2 X + b_3 T + c_1 TR_1 + c_2 TR_2 + c_3 TR_3 + c_4 TM + e_3$$

where: S_i = site dummy variables[17]

R_i = racial/ethnic dummy variables[18]

M = a dummy variable that equals 1 for males

By using equation (2) or (3), the estimated impact on any individual can be calculated. A subgroup impact presented in the report, such as that for Hispanics, is the average of all individual subgroup members' impacts, assuming subgroup members differ from the rest of the sample only by being a member of that

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[16] The proposed correction involves a two-stage estimation technique. In the first stage, sample inclusion is modeled. Using the probit coefficients from the first stage, the inverse Mill's ratio is constructed as:

$$T \frac{f(Z_i b)}{F(Z_i b)} - (1-T) \frac{f(Z_i b)}{1-F(Z_i b)}$$

where T is the dummy variable indicating treatment status; F(Z) and f(Z) are the cumulative distribution and density functions of the standard normal function; Z_i is a row vector of explanatory variables that affect sample attrition for person i; and the b's are estimated probit coefficients. The inverse Mill's ratio is then included in all regression models using the given sample. See Heckman (1979) and Amemiya (1981) for further discussion of this methodology.

[17] One site--Seattle--is omitted from the equation.

[18] One racial/ethnic group category--whites and others--is omitted.

subgroup, i.e., their other characteristics are, on average, the same as for the overall sample.[19]

The use of OLS is not warranted when the dependent variable is dichotomous, such as whether a participant uses contraception during the program period or becomes a school dropout.[20] In such cases, a nonlinear maximum likelihood estimation technique, logit, is used to estimate treatment effects.

The key finding of the analysis is whether STEP has an effect on various outcome measures. In the discussion of the results, we indicate whether an impact estimate is statistically different from zero by labeling statistically non-zero estimates as "significant." In this report, the term is reserved for estimates that are not equal to zero at a 0.10 or greater level of significance.

When discussing subgroup estimates, a second finding is also of interest: whether the effect of STEP differs with respect to a particular characteristic, such as race or ethnicity. A test of whether the subgroup impacts differ from one another was conducted for all subgroup analyses.

In summary, a variety of analytic strategies are used to evaluate the impact of participation in STEP. The fundamental approach uses a dummy variable (indicating treatment or control group status) in OLS regression. Other analyses are used where the assumptions of the OLS model are likely to be violated; where dependent measures are jointly determined; where the measurement of the treatment variable needs refining; and where program impacts for selected subgroups of participants are examined.

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[19] This estimate is calculated as an appropriately valued linear combination of treatment and treatment-interaction coefficients. For example, the estimated impact on subgroup R_1 is: $b_3 + c_1\ 1 + c_2\ 0 + c_3\ 0 + c_4$ (the average percentage of males among all treatments).

[20] See Amemiya (1981) for details about the problems involved in estimation with dichotomous variables.

IV. PROGRAM IMPLEMENTATION

As a year-round program model combining summer remediation, life skills instruction, work experience and school-year support activities, STEP requires well-formulated strategies for successful implementation. This chapter discusses the requirements for STEP implementation; describes the strategies utilized by sites in providing services; identifies problems encountered and assesses solutions attempted; and provides recommendations for best implementation practices. Key issues and considerations for program planning are examined, including choosing between morning and afternoon remediation, single-site or multiple-site programs, the role of a lead teacher, developing half-time jobs, and staffing considerations. While Year Three operations are the focus for these discussions, references to operations in the first two demonstration years are made where appropriate. Readers are referred to the reports on the 1986 and 1985 operations for greater detail.

Collaboration between educational institutions and employment and training organizations is essential to implementing the STEP model as designed. School systems, Private Industry Councils and city agencies must work cooperatively toward achieving the simultaneous goals of improving participants' prospects for high school graduation and preparing them to enter the labor market. The discussions that follow will illustrate that effective collaboration is key to successful program implementation. School systems and other agencies must jointly decide on the population to be served; schools must develop criteria for identifying educationally at-risk youth and give approval for the program to grant academic or elective credit; and JTPA agencies must incorporate academic screens into their typical program eligibility process. In addition, payroll systems that count classroom and work hours separately may need to be devised; special projects that provide part-time jobs for youth may need to be developed; and schools may need to adjust rules to give SYS access to youth during the school day.

In the five demonstration sites many of the agencies who became involved in STEP had worked together prior to STEP. However, the intensity and complexity of the STEP model still represented a challenge for most sites. Collaborative efforts improved with each year of the demonstration. In both Portland and Seattle, for example, the classroom components of the program were managed exclusively by the JTPA agency during the first program year--by DHR in Seattle and the PIC in Portland. However, in the second and third years of the demonstration, the school districts in both sites became more involved with the program and assumed greater responsibility for remediation and life skills instruction. The roles and areas of responsibility assumed by each of the STEP agencies in the five demonstration sites during the third year of the demonstration are shown in Table IV.1.

The mix of agencies that provide funding for STEP in demonstration sites further illustrates the level of collaboration that has developed. In addition to the P/PV grant for the demonstration, several other funding sources are necessary to run STEP. In Boston, funds are provided by the JTPA agency, ABCD, and the Mayor's Office of Jobs and Community Services; the four universities that serve as remediation sites also make in-kind contributions. Fresno's program is funded by the Fresno PIC and the Fresno County Economic Opportunities Commission; in-kind contributions are received from the Fresno Unified School District. In Portland, STEP is funded by JTPA's Title II-A and II-B funds, the Portland PIC and in-kind contributions from the Portland Public Schools. San Diego's program receives funding from the San Diego Unified School District, the Regional Employment and Training Consortium, and the Regional Youth Employment Agency--the local JTPA agency. Finally, in Seattle, the State Board for Vocational Education, the PIC, the United Way and in-kind contributions from the Seattle Public Schools and the Department of Human Resources support the program.

YEAR THREE

Building on the experiences of the first two demonstration years, three STEP sites modified their program structures for the third year of the demonstration. These changes were within the guidelines of the STEP model. They included the elimination of afternoon remediation sessions in Seattle, the reduction from five remediation sites to two in San Diego, and the addition of a fourth remediation site in Boston to enhance local expansion of the program. Structures were unchanged from the previous summer in Fresno and Portland.

In Year Three, cross-site uniformity increased, largely because of the Seattle change. For the first time, all STEP sites offered remediation during the morning hours only. Site experience with the components, a high percentage of returning teachers and no major design changes (like the curriculum changes in Year Two) also contributed to the increased uniformity and smoother implementation across sites.

Consistent with model specifications, STEP sites provided a minimum of 90 hours of basic skills remediation and 18 to 20 hours of life skills instruction. Treatment youth worked an average of 94 hours during the third year of the demonstration, while control youth, who worked full time, participated for an average of 190 hours.

Table IV.1

LOCAL OPERATING AGENCIES

		SERVICE PROVIDERS BY PROGRAM COMPONENT			
SITE	**LEAD AGENCY**	**REMEDIATION**	**LIFE SKILLS**	**WORK EXPERIENCE**	**SCHOOL-YEAR SUPPORT**
Boston, MA	Mayor's Office of Jobs and Community Services (MOJCS)	ABCD at: Boston University Harvard University Northeastern Univ. UMass - Boston	ABCD at: Boston University Harvard University Northeastern Univ. UMass - Boston	ABCD at: Boston University Harvard University Northeastern Univ. UMass - Boston	Mayor's Office of Jobs and Community Services (MOJCS)
Fresno, CA	Fresno Private Industry Council (FPIC)	Fresno Unified School District (FUSD)	Fresno Unified School District (FUSD)	Fresno County Economic Opportunities Commission (FCEOC)	Fresno Unified School District (FUSD)
Portland, OR	Portland Private Industry Council	Portland Private Industry Council and Portland Public Schools	Portland Private Industry Council	Portland Private Industry Council	Portland Private Industry Council
San Diego, CA	San Diego Regional Employment & Training Consortium (RETC)	San Diego City Schools	San Diego City Schools	San Diego Regional Youth Employment Program (REGY)	San Diego City Schools
Seattle, WA	Seattle Department of Human Resources (SDHR)	SDHR and Seattle Public Schools	SDHR and Seattle Public Schools	Seattle Department of Human Resources (SDHR)	Public/Private Ventures (P/PV)

The activities associated with selecting and serving the third cohort of STEP youth, reenrolling Cohort II youth and operating the program components are discussed in the remainder of this chapter, beginning with participant selection and assessments.

PARTICIPANT SELECTION AND ASSESSMENTS

Recruiting a New Cohort

Initially, plans for the STEP demonstration included providing services to two cohorts of youth only. However, as discussed in Chapter II, the introduction of the new Practical Academics curriculum in Year Two and the desire to study the two-summer intervention in its most uniform implementation mode prompted the addition of a third cohort of youth.

Defining the target population within the confines of the STEP model and thus determining the academic criteria for selection is one of the first decisions that any site considering instituting a STEP program must make. Sites must carefully define the group(s) they wish to target and establish strategies for recruiting students who meet program criteria to ensure that a program attracts and serves those group(s).

The initial eligibility criteria for STEP--a chronological age of 14 or 15 on the first day of the program, JTPA eligibility and academic performance that indicates a high risk of dropping out (at least one year below expected grade level in reading or math)--remained the basis for selecting the third cohort of STEP participants. As with other cohorts, Cohort III youth were required to read at no lower than a fourth-grade level to ensure that they have the ability to benefit from the program's curriculum materials.

Differences in the standardized testing practices at each of the participating school districts (different tests are used and administered at different times) necessitated local interpretation of the generic STEP eligibility definition. These interpretations, presented in Table IV.2, remained unchanged from Year Two of the demonstration.

Attesting to the much publicized educational crisis in American schools, each site identified a surplus of youth showing academic deficiencies. In Boston, 2,650 academically eligible 14- and 15-year-olds were identified; 2,754 youth were on Fresno's list; 1,974 were on Portland's; approximately 3,000 were on San Diego's and 1,573 were on Seattle's.

With these numbers of academically eligible youth in each site, it is almost inconceivable that recruiting adequate numbers of

Table IV.2

ACADEMIC ELIGIBILITY MEASURES BY SITE

Site	Primary Academic Measures	Secondary Measures
Boston	MAT score between the 20th and 40th percentiles in reading or math and/or Degree of Reading Power Test score of 25 to 47	Preference initially given to 8th-graders
Fresno	At least one year below current grade level in reading or language or math as determined by the California Achievement Test (CAT)	None
Portland	At least one but not more than four years below grade level in reading and/or math using the Portland Achievement Levels Test (PALT)	None
San Diego	One to four years behind the grade norm using the Comprehensive Test of Basic Skills (CTBS)	None
Seattle	One grade level below current grade in reading or math using the California Achievement Test (CAT) or the Wide Range Achievement Test (WRAT)	Retained in the past five years

participants could have been a problem; however, it was. Despite sites' familiarity with the mechanics of the model, achieving the recruitment goal was as challenging a task in Year Three as it had been in previous demonstration years and required the same level of effort. One problem is that youth generally tend to wait until the summer is upon them before they apply for summer jobs. Also, many youth on schools' lists do not meet JTPA requirements; others do not want to participate in a part-work/ part-school program. In at least one site, the new Immigration Reform and Control Act requiring youth to document citizenship with either a birth certificate or "green card" is believed to have deterred youth from applying. These problems were all compounded by P/PV's relatively late decision to serve a third cohort of youth.

These difficulties underscore the need for early and intensive outreach and recruitment efforts to achieve enrollment goals. Lists of academically eligible youth were developed by school districts as early as January and February. While such early identification allows utilization of targeted approaches to recruitment and outreach, it does little to modify the summer job application pattern of the majority of the targeted youth or to eliminate the other barriers to enrollment.

Direct mail, telephone calls, meetings at schools, the involvement of school counselors and the distribution of flyers at community locations were among strategies used to attract youth to STEP. In Seattle, parents were found to be a strong ally in recruiting for the previous cohorts; as a result, Seattle's 1987 recruitment strategy featured direct mail and telephone contact with parents rather than direct contacts with youth. In combination, such strategies helped produce the required sample size in each site.

Reenrolling Cohort II

Along with recruiting a new cohort of youth for STEP in 1987, sites were charged with the task of reenlisting Cohort II youth for a second summer of STEP. Treatment youth in all sites were invited to participate in a second summer, unless deemed by site staff to present a negative influence on the program. In two sites, San Diego and Seattle, controls were also offered a second summer of work experience. San Diego has offered two summers of work to controls since the beginning of the demonstration; 1987 was the first summer during which Seattle made such an offer.

Despite initial expectations that youth would be reluctant to return to a summer jobs program with an educational component, the return rates of treatments and controls indicate no difference in motivation or program interest. As shown in Table IV.3, 611 Cohort II treatment youth, or 75 percent of the randomized treatment sample, returned to STEP in 1987. In Seattle and San

Table IV.3

REENROLLMENT OF COHORT II YOUTH IN 1987

Site	Number of Treatment Youth Randomized in 1986	Number (Percentage) of Treatments Reenrolled in 1987	Number of Control Youth Randomized in 1986	Number (Percentage) of Controls Reenrolled in 1987
All Sites	819	611(75%)	816	242(74%)
Boston	166	105(63%)	165	N.A.
Fresno	164	131(80%)	164	N.A.
Portland	163	130(80%)	162	N.A.
San Diego	161	116(72%)	160	122(76%)
Seattle	165	129(78%)	165	120(73%)

N.A. = not applicable (Control youth were not offered a second summer of work experience in these sites.)

Diego, where controls were offered the opportunity to return, control return rates were 73 and 76 percent, respectively. Slightly more controls returned to the San Diego program than did treatments, while the opposite was true in Seattle.

Stronger programs during the 1986 summer and a stronger SYS program across sites did not, however, improve the return rates in Year Three. Return rates for both treatments and controls were lower in Year Three than in Year Two, when treatment youth had an 80 percent return rate and control youth in San Diego had an 89 percent rate of return. All sites reported a difficult reenrollment effort, saying that the process was both labor-intensive and time-consuming.

Some reenrollment difficulties in Boston and San Diego stemmed from healthier local economies, which led to stronger competition for summer employees. Many of these non-STEP opportunities were in the private sector, where youth could earn more money than they could through STEP. As was the case in 1986, some youth in all sites needed to make up specific courses and were lost to traditional summer-school programs.

Contact during the school year was a major element of the reenrollment strategy. Participants in the SYS component of STEP were frequently reminded of their opportunity to participate in a second summer of STEP, and a variety of back-to-summer activities were hosted to encourage reenrollment and assist youth in completion of program applications. However, the decision to return appears to have been influenced by the SYS component only when the level of contact was high. A review of SYS contact data revealed that among youth who returned, 75 percent had high levels of contact (one or more contacts in five or more months of the school year), while only 6 percent received no SYS contact. Low levels of SYS contact (at least one contact in no more than four months of the school year) did not seem to affect the decision to return--53 percent of the returnees had low levels of contact, compared with 47 percent of the non-returnees.

Testing and Questionnaire Administration

As part of the STEP research design, youth are tested to determine their reading and math skills at the beginning and end of each summer of participation. As a result of amendments made to JTPA legislation in 1986, assessment of participants' basic skills is now required in the larger SYETP. Programs, therefore, must either select an assessment tool for use in their programs or access school records to determine eligibility.

The Survey Battery, Intermediate Level of the Metropolitan Achievement Test (MAT), was selected as the assessment tool for STEP youth. Forms JS and KS are used--one as a pretest and the other as a posttest. Only the math and reading sections of the survey are used.

The MAT was selected for several reasons. First, the MAT is familiar to schools and is regarded as a reliable testing instrument. Second, the population on which the test is normed is similar in age to the STEP population. Third, the test assesses academic rather than functional employability skills and is therefore consistent with the objectives of the remediation component--to improve reading and math skill levels.[21] Fourth, the length of time required to complete the battery was not obtrusive to operation of the program. Finally, use of the MAT allows comparison to a national population.

Procedures for testing youth have remained fairly consistent since the beginning of the demonstration. In general, youth are tested either prior to or on the first morning of the program, with make-up testing occurring during the first two program weeks as needed. Treatment and control youth are tested under the same conditions and generally at the same time. New program youth are not apprised of their group assignments until after the test is completed. These procedures have been followed in four of the five sites. In Fresno, local restrictions on administering the questionnaire on school grounds necessitated different arrangements. Treatment and control youth in Fresno were pretested in separate afternoon sessions a few days prior to program start with the same rooms and proctors used. However, posttesting was more uniform across sites, due to a shift to morning posttesting in Fresno.

P/PV employed independent test observers to witness the testing process and report their observations by completing structured instruments. These instruments, along with reports completed by P/PV's site coordinators, indicate that testing conditions were uniform both within and across sites. Since treatments and controls were generally tested together, the testing process did not differ by group. In general, testing conditions were satisfactory and proctors followed procedures as instructed.

Observers reported that most youth took the test seriously, but noted that youth seemed to be less attentive at posttest than at pretest in four of the five sites. In Boston, however, attentiveness was reported to be slightly better at the posttest than at the pretest.

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[21] No test has been designed specifically for the STEP Practical Academics curriculum; thus, no test is perfectly suited to its content and approach. However, since Practical Academics is designed to teach and reinforce students' basic skills in reading and math, its general goal and the specific skills that are covered are compatible with the skill specifications of the MAT.

Sites were successful in achieving a high turnout for testing. As Table IV.4 shows, pretests were conducted with 91 percent of both the reenrolled Cohort II youth and youth randomized to Cohort III. Posttest attendance was similarly high--93 percent of pretested Cohort II youth were posttested and 94 percent of pretested Cohort III youth were posttested. These numbers have remained fairly consistent in each year of the demonstration.

THE REMEDIATION COMPONENT

Standardized test scores indicate that a high percentage of STEP youth are behind their grade of enrollment in reading and math skill achievement. STEP's remediation curriculum is designed to improve these two basic skill areas. Treatment youth receive 90 hours of remediation, typically provided in daily sessions of from three to three and one-half hours over a six- or seven-week period. Table IV.5 presents a sample weekly schedule. Computer-assisted instruction (CAI) that constitutes 20 to 25 percent of total instructional hours and a teacher/student ratio of at least 1:15 are also integral to STEP's remediation model.

The Practical Academics curriculum, developed in Year Two of the demonstration, was again the primary tool for remedial instruction during Year Three. Practical Academics is a scripted curriculum that incorporates teacher-led group instruction, student-paced skill reinforcement exercises, supplementary projects (both group and individual), and independent reading and writing assignments. Twenty minutes of sustained silent reading is also part of the daily remediation experience. This curriculum approach was selected because it combines direct instruction with focused practice using motivating materials. Topic-based learning modules about issues that are relevant and interesting to STEP youth provide the motivation and the context for skill instruction.

The modules, which form the nucleus of the curriculum, are self-contained teaching guides. Each one includes a lesson script for each topic; applied activity sheets for use by students, with answer keys for teachers; copies of required handouts; and a list of suggested skill reinforcement activities. P/PV has made a deliberate effort to address higher-order thinking skills in the modules, such as predicting, inferring and problem-solving, and has suggested numerous activities and projects that promote wider communication skills, especially writing.

Table IV.4

YOUTH TESTED BY SITE AND COHORT

	All Sites	Boston	Fresno	Portland	San Diego	Seattle
Cohort III						
Number Randomized	1,591	330	315	342	329	276
Number (%) of Randomized Sample Pretested	1,457(92%)	288(87%)	288(91%)	319(93%)	301(91%)	261(95%)
Number (%) of Pretested Sample Posttested	1,368(94%)	274(95%)	277(96%)	295(92%)	268(89%)	254(97%)
Cohort II						
Number of Treatments Randomized in 1986	819	166	164	163	161	165
Number (%) of Treatments Reenrolled	611(75%)	105(63%)	131(80%)	130(80%)	116(72%)	129(78%)
Number (%) of Reenrollees Pretested	559(91%)	96(91%)	122(93%)	112(86%)	107(92%)	122(95%)
Number (%) of Pretested Reenrollees Posttested	519(93%)	82(85%)	108(88%)	109(97%)	98(92%)	121(99%)

Table IV.5

SUMMER TRAINING AND EDUCATION PROGRAM
SAMPLE WEEKLY SCHEDULE

	MONDAY	TUESDAY	WEDNESDAY	THURSDAY	FRIDAY
MORNING	MODULE INSTRUCTION (Topic 1) o Group motivational activity o Teacher-led skills instruction o Applied activities o Summary of lesson INDIVIDUAL INSTRUCTION o Skills reinforcement in reading/math and/or o Independent projects o Computer-assisted instruction o Sustained silent reading	LIFE SKILLS AND OPPORTUNITIES INDIVIDUAL INSTRUCTION	MODULE INSTRUCTION (Topic 2) INDIVIDUAL INSTRUCTION	LIFE SKILLS AND OPPORTUNITIES INDIVIDUAL INSTRUCTION	MODULE INSTRUCTION (Topic 3) INDIVIDUAL INSTRUCTION
LUNCH BREAK	LUNCH	LUNCH	LUNCH	LUNCH	LUNCH
AFTERNOON	WORK EXPERIENCE	WORK EXPERIENCE	WORK EXPERIENCE	WORK EXPERIENCE	WORK EXPERIENCE

Within each module, initial instruction is designed for presentation to the whole class at a mid-range level (between seventh and eighth grades). However, applied activities have been developed for students who read at levels ranging from fourth- to 10th-grade. Levels of difficulty are also differentiated by the complexity of the skills that students use and practice.

Modules can be interchanged between cohorts. The only restriction is that the modules used in the first year of the program cannot be used with the same youth during their second summer. Also, there are some modules that are recommended for a specific cohort because they complement the content of material being covered in the LSO curriculum. During Year Three, the following modules, whose focus encompassed school-based topics, employability skills and life skills, were used: Your Summer Job, Jobs, Self-Identity, The Literature Connection, The Drama Connection, Newspapers, Legal Rights, Substance Abuse, Our USA and Decisions.

In Year Three, individual modules were refined based on feedback from teachers. Module refinements included the substitution of a continuum of activities that become progressively more difficult for activities that encompassed only three levels of difficulty, as well as the addition of writing activities related to comprehension and an improved formatting of materials for both teachers and students. Also, five new modules were added to the curriculum, bringing the total number of P/PV-developed modules to 14. Individual sites have also developed five modules locally.

Remediation Schedule

As noted earlier, remediation was only offered during morning hours at all five sites during the third demonstration year. Previously, the Seattle site offered remediation to youth on a dual schedule that included both morning and afternoon sessions. The decision to switch to morning-only remediation came after two summers of operating a dual system. Logistical and management problems encountered in a dual schedule system, such as keeping track of students' schedules, teacher and student fatigue in the afternoon and complaints by youth about the need to come to school dirty (for those who held custodial/maintenance jobs), were important considerations for program operators in making this change.

The benefits of a morning-only program are numerous. First, while a site can hire fewer teachers in a dual schedule system, the cost of hiring twice as many teachers is often no more than the cost of hiring fewer teachers for double shifts. Second, students and teachers tend to be more alert in the morning than in the afternoon when they have already put in a half-day on the job. Third, student attendance is likely to improve and enforcement of the no school/no work sanction[22] is likely to be easier. Fourth, the difficulty of teaching a new curriculum is reduced when the day is shorter and teachers, who tend to be exhausted at the end of the day under a dual system, have more time to familiarize themselves with materials. Management of the remediation component may also be improved with a morning-only program. Staff meetings are more easily arranged, since they can be held at the end of the day. Morning sessions may also improve group cohesion among students, since all participants attend classes at the same time.

While some resistance from students regarding a half school/half work summer jobs program was experienced in recruitment and re-enrollment efforts for STEP, many students like the idea. In questionnaires completed at the end of the summer, almost half (49%) of the Cohort III treatment youth like the half school/half work schedule of STEP. Another 45 percent of these youth would prefer to work all day, while the remaining 6 percent would like to attend classes all day. While a majority (72%) of Cohort III control youth said they would prefer working all day, almost one-fourth (23%) said they would like to attend classes for half a day. As is the case with treatment youth, 6 percent of Cohort III controls would prefer to attend classes all day.

Among Cohort II returning youth, over half (55%) would prefer to work all day while 37 percent like the dual program schedule. Eight percent would prefer to attend school all day.

Teacher Training and Class Preparation

One area of focus in planning the Year Three program was to improve teacher training at the sites. To that end, P/PV developed a series of training guides in special need areas. The five training guides cover effective instructional principles; the effective use of instructional aides; classroom management and positive student behavior; sustained silent reading; and computer-assisted instruction. Along with lead teachers and other local staff, P/PV's remediation consultants and staff delivered pre-service training at all sites.

Beginning in late spring, teachers received an average of between 15 and 20 hours of pre-service training. (P/PV's guidelines had recommended that approximately 28 hours of training be provided.) The hours devoted to pre-service training varied by site.

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[22] An important feature of the STEP model is the requirement that youth attend both classes and the work portion of the program to receive payment. Sites have developed strategies to inform worksites about remediation absentees so that these youth are not allowed to work that afternoon. This strategy encourages attendance at the school portion of STEP.

Teachers in Seattle report receiving the most training, between 26 and 30 hours on average. Fresno's teachers were second, with an average of between 21 and 25 hours reported. In the other sites, teachers reported receiving between 15 and 20 hours of training.

In addition to the specific topics outlined in the training guides, teacher training included an orientation to the program; discussions on the integration of the LSO and work experience components; review of modules; and a review of program logistics, including time cards, discipline, grading and room set-up. While 51 percent of teachers indicated that a sufficient amount of pre-service training was received, 49 percent of the teachers said they felt more training was needed. Seventy-nine percent of the teachers felt that the quality of training was good or excellent.

In addition to pre-service training, teachers also received in-service training over the course of the summer program. Much of this training took place in staff meetings, where the lead teacher and remediation consultants provided technical assistance and addressed ongoing issues of concern. Ninety percent of the teachers received such training. Across sites, teachers reported receiving an average of four to six hours of in-service training, with the amount reported again varying widely by site--ranging from between four and six hours in San Diego to between 10 and 12 hours in Portland. STEP teachers again gave a high rating to the quality of training, but 36 percent of respondents felt that an insufficient amount of in-service training was provided.

Teachers also spent considerable time in preparation for remediation classes. Across sites, teachers reported spending an average of approximately six hours each week in classroom preparation activities. Very little variation in reported preparation time was found in four of the five sites. In Boston and Portland, teachers reported spending an average of 6.6 hours each week; in San Diego, teachers spent an average of 6.5 hours; and in Seattle teachers spent 6.1 hours in class preparation on average. In Fresno, however, teachers reported spending an average of 5.1 hours on preparation activities--a full hour less than in the next lowest site.

Staffing Patterns and Teacher Characteristics

As in Year Two, each site hired a lead teacher. Lead teachers are primarily responsible for overseeing the remediation component but may also assume responsibility for seeing that LSO staff are supported and successfully integrated into program operations. The definition of the lead teacher role varied from site to site with respect to life skills, but the role was consistent in regard to remediation.

72

Lead teachers are typically hired in February or March, and must assume responsibility for assembling a curriculum development team of reading and math teachers to develop and/or revise curriculum modules for local use; selecting print and CAI materials for skill reinforcement; hiring teachers; planning and providing teacher training; and managing the component during the summer. Lead teachers work full time during the summer, but they do not assume a teaching load.

The role of lead teacher is integral to the success of the model and this staff inclusion is strongly recommended to any site considering a summer remediation program. The planning process required for a summer program benefits from enlisting a lead teacher as early as possible, and from the lead teacher's gaining an early understanding of program objectives. A central authority to whom instructional issues can be referred as they arise has proved an effective and efficient use of program resources.

In addition to the five lead teachers, 82 teachers were hired to staff the five sites' remediation component for Summer 1987. The number of teachers hired in Year Three was the highest to date, due primarily to the switch to morning-only remediation in Seattle. Boston hired 14 teachers, Fresno hired 12, Portland and Seattle each hired 18, and San Diego hired 20. Selected characteristics of remediation teachers in each site are presented in Table IV.6.[23]

The number of teachers hired by a site depends on the number of youth who returned for a second summer, the number of subsites used and the configuration of assignments--that is, whether teachers will teach both reading and math or separate teachers will be hired for each subject area. In Boston and Seattle, remediation teachers provided instruction in reading, modules and math, and CAI experts were hired to staff computer labs. In Fresno, separate teachers were hired to provide reading, module and math instruction with reading and math teachers assuming responsibility for their respective CAI component.[24] In Portland, teachers were assigned by specialty to reading, module classes or math/CAI classes. San Diego also employed separate teachers for reading and math, with reading-related CAI taught by reading teachers and math-related CAI taught by math instructors.

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[23] Although there were a total of 82 remediation teachers across sites, two of Boston's teachers did not complete questionnaires. The teacher data cited in this chapter, therefore, are derived from only 80 instructors.

[24] On Fridays, LSO teachers in Fresno team-taught with module instructors.

Table IV.6

REMEDIATION TEACHER CHARACTERISTICS BY SITE

	All Sites	Boston	Fresno	Portland	San Diego	Seattle
Gender						
Female	44(55%)	9(75%)	6(50%)	5(28%)	10(50%)	14(78%)
Male	36(45%)	3(25%)	6(50%)	13(72%)	10(50%)	4(22%)
Race/Ethnicity						
Asian	3(4%)	1(8%)	1(8%)	0	0	1(6%)
Black	24(30%)	6(50%)	1(8%)	6(33%)	2(10%)	9(50%)
Hispanic	6(8%)	1(8%)	3(25%)	0	2(10%)	0
White	45(56%)	2(17%)	7(59%)	12(67%)	16(80%)	8(44%)
Other	2(2%)	2(17%)	0	0	0	0
Program History						
1st-Year Teacher	39(49%)	3(25%)	4(33%)	11(61%)	5(25%)	16(89%)
2nd-Year Teacher	28(35%)	6(50%)	5(42%)	5(28%)	10(50%)	2(11%)
3rd-Year Teacher	13(16%)	3(25%)	3(25%)	2(11%)	5(25%)	0
Teaching Experience						
Mean Years of Experience	12	12	11	10	14	13
Cohort Assignment						
Cohort II	35(44%)	4(33%)	6(50%)	9(50%)	8(40%)	8(44%)
Cohort III	40(50%)	5(42%)	6(50%)	9(50%)	12(60%)	8(44%)
Both Cohorts	5(6%)	3(25%)	0	0	0	2(11%)
Sample Size	80	12	12	18	20	18

P/PV's guidelines for teacher selection include requiring a background in reading or math and experience with disadvantaged populations. Sites also attempted to hire a group of racially and ethnically diverse individuals in order to achieve greater congruence with the student population. Past experience and performance with STEP were also important factors in hiring decisions.

The teaching staffs included many teachers who had taught in the program in an earlier summer; 35 percent had taught in STEP in one of the two previous summers, and 16 percent were teaching in STEP for their third summer. On average, teachers had 12 years of teaching experience, with site averages ranging from 10 years in Portland to 14 years in San Diego. Half of the teachers were assigned to the Cohort III group exclusively, 35 taught only returning Cohort II youth and five taught both groups of youth.[25]

Sites were able to hire a group of teachers whose expertise was in reading and math; 77 percent had taught reading/English or math prior to STEP. STEP teachers were also experienced with disadvantaged populations, with 86 percent reporting prior experience with special needs populations. While teachers had taught at grade levels ranging from the elementary school to high school levels, most taught at the middle/junior high or high school level in the school year preceding STEP. Most remediation teachers--66 percent--had received master's level degrees, while 28 percent had bachelor's degrees only. Sites also obtained a slightly more diverse racial/ethnic mix in the teachers they hired in 1987 than they had in Year Two. Fifty-six percent of STEP teachers were white, 30 percent were black, 8 percent were Hispanic, 4 percent were Asian, and 2 percent came from some other background.

Fifty-five percent of the teaching staff across sites were female. More females than males were hired in Boston and Seattle, while the opposite was true in Portland. Both Fresno and San Diego hired an equal number of males and females.

The efforts of teachers were complemented by instructional aides in all sites. Seventy-seven of the 80 remediation teachers who completed questionnaires reported that they had access to an instructional assistant. In some cases, an aide was shared among several teachers; in others, aides were assigned exclusively to a single instructor. Instructional assistants performed a variety of teaching tasks, including direction of small groups, tutoring and providing feedback to students. They also handled clerical tasks, such as taking attendance and grading assignments.

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[25] Teachers assigned to both cohorts were hired in Boston and Seattle only and taught CAI sessions.

Instructional assistants attended specified portions of the teacher training sessions to improve their understanding of program goals and their role in achieving them. Those sessions also served to familiarize assistants with materials and the teaching staff to whom they were assigned; and to enhance their perception of themselves as integral to the remediation program.

As will be discussed in Chapter VI, no measurable teacher characteristic showed an impact on students' performance on the MAT posttest. However, P/PV asked site coordinators to identify the qualities they felt were most essential for STEP teachers. According to P/PV's site coordinators, teachers with the following qualities had the most success in the classroom:

- Willingness to deliver the STEP curriculum;
- Teaching experience and familiarity with the population;
- Organization and diligence in preparation;
- Compassion and sensitivity for the needs of the students;
- Willingness to put in extra, unpaid hours;
- Experience in curriculum adaptations;
- Ability to function as a member of a team;
- Willingness to function in a manner that may be different from the school year;
- Ability to structure and organize the class with compelling but not oppressive authority;
- Ability to maintain a positive atmosphere that nourishes self-esteem and yet maintains discipline and time-on-task;
- Ability to generate enthusiasm for learning; and
- Ability to teach a self-paced curriculum.

Observations and Assessments

For the most part, teachers across sites, three-fourths of whom had used a skills-based, individualized curriculum prior to their affiliation with STEP, approved of the instructional approach used. In fact, 50 percent said they believe STEP's method of instruction is better than traditional methods. Sixty percent of the teachers reported that they liked the modules somewhat, while

37 percent reported liking modules very much. Teachers' feedback on individual modules indicated that the modules generally met their stated instructional goals. However, many reported that too many activities were required for the allotted time or that the teacher's script was too detailed. While only 45 percent of the teachers were familiar with CAI beforehand, 96 percent found it an effective instructional tool. Finally, while 67 percent of the teachers felt that the length of the STEP day was appropriate, 14 percent felt it was too long and 19 percent felt it was too short.

In addition to program implementation assessments provided by teachers, P/PV's on-site coordinators and central office staff conducted structured classroom observations of both the remediation and LSO components. These observations served as a program monitoring tool and provided a hands-on understanding of the typical STEP classroom experience. A total of 200 remediation observation instruments were completed.

Classroom observations indicate that an aide assisted the instructor in 88 percent of the STEP classes, which had an average of 14 youth present. Youth participated in the intended variety of activities--module instruction, applied activities, CAI, independent work, silent sustained reading and skill reinforcement. Forty-three percent of the observations identified individualized skills practice as the primary mode of instruction. This was followed by skills practice for the full group (21%), direct group instruction (17%) and CAI (11%). Direct individual instruction and other unspecified forms of instruction combined to fill the remaining 8 percent of observed instructional time.

Observers indicated that 63 percent of module instruction classes were primarily devoted to the development of academic skills, while 18 percent focused on the content of the material. Instructional focus was equally split between content and skills in 19 percent of observed classes. Student response to the remediation classes was very favorable; youth were reported to be interested in the lesson in 75 percent of the observations and were described as attentive in 94 percent of the observations. Quality of instruction was also rated quite favorably by classroom observers. In almost three-fourths of the observations (74%), instruction received a better than average rating.

On questionnaires completed at the end of the summer, students also rated the remediation experience favorably. Sixty-four percent of returning Cohort II youth indicated that they learned much and 27 percent said they learned some by being in STEP. Forty-five percent of these youth felt that the program helped them equally in both reading and math, while 14 percent felt that its major benefit was in reading and 12 percent felt it helped most in math. Nearly one-third of Cohort II youth felt they were doing better in school after STEP than they had done before they enrolled in STEP. Among treatment youth from Cohort III, 43 percent found the classroom portion of STEP more interesting than regular school, as did 31 percent of the returning treatment youth from Cohort II.

Incentives for Participation

Two types of incentives are available for promoting participation among youth--the provision of academic credits and financial payment. The granting of credits is believed to be an important feature of the program for many youth who are behind in the number of credits needed for graduation. School credits are granted in several sites for satisfactory completion of the summer program. The type of credits granted in Year Three and the conditions under which they were granted differed from site to site. In Boston, youth could receive academic credit in reading, math or English for completing STEP only if they had failed one of these courses; the credit was granted in lieu of summer school and thus did not represent "extra" credit. In Fresno, students received "extra" academic credits in reading and/or math, in addition to a work experience credit that was granted to both treatments and controls. In San Diego, all youth were granted work experience credit only. Youth in Seattle received academic credit in reading and/or math. No credits were granted to youth in the Portland program.

STEP youth in all sites were paid the minimum wage, not only for work projects but for their participation in remediation and LSO classes as well. Since all STEP youth are economically disadvantaged, the summer is a critical period that represents a time when they can earn money. The decision to pay for remediation was based on the belief that paying youth only for work would decrease their earning potential by half from what they could earn in a work-only program, thus levying an unfair penalty on them. A significant number of youth might also be lost to full-time jobs if payment for remediation was withheld.

Attendance Rates

In Year Three of the demonstration, Cohorts II and III treatment youth were offered total classroom time ranging from 108.5 hours in Seattle to 131.75 hours at two of the four Boston subsites. In Fresno, youth were offered 115.5 hours; in Portland, 112.5 hours; and in San Diego, 114 hours. For the purpose of payroll, time spent in remediation and LSO classes is counted together and constitutes "classroom hours." Therefore, it is not possible to determine precisely what proportion of classroom time is LSO and what proportion is remediation. However, LSO accounts for a maximum of 20 hours of total classroom time.

Across sites, youth participated in classroom activities an average of approximately 100 hours or 85 percent of available classroom time. San Diego's youth had the highest rate of attendance in remediation classes--89 percent for Cohort III and 88 percent for Cohort II. At the other end of the spectrum were youth in Boston and Portland, where Cohort III youth were present for only 83 percent of available hours, and Cohort II youth posted an 82 percent rate. The rate of classroom attendance was consistent with the Year Two figures for first-year youth in San Diego--89 percent. Year Three participation was also fairly consistent with the Year Two level in Boston, decreasing by only two percentage points. Meanwhile, participation decreased in Fresno by five percentage points and improved greatly in Portland and Seattle--up 10 and 12 percentage points, respectively. As Table IV.7 shows, treatment youth from both cohorts had higher participation rates in classroom activities than in work experience activities. This was true in all sites except San Diego.

Summary

The remediation component was successfully implemented and was staffed by fully qualified teachers in the third year of the STEP demonstration. Observation reports indicate that the curriculum was generally delivered as designed and instruction was indeed individualized. Teacher response to the use of modules was positive, and their suggested revisions to modules were minor.

The switch to morning-only remediation classes in Seattle enhanced the uniformity of implementation across sites, as did the decrease from five remediation subsites to two in San Diego. The number of teachers who were familiar with the program's goals, materials and instructional methods due to prior STEP experience further added to the success of implementation.

Finally, observers' reports of the level of attentiveness in the classroom indicate that youth have a serious attitude toward the program. Student responses on questionnaires administered at the end of the summer show that youth have a positive perception of program benefits.

THE LIFE SKILLS AND OPPORTUNITIES (LSO) COMPONENT

Along with poor academic skills, pregnancy or fathering a child can be key factors in a teenager's decision to leave school prior to attaining a high school diploma. The LSO component of STEP informs youth about the implications of early parenthood for educational and economic goals and provides information about responsible sexual behavior. This information is especially important to STEP youth, who though frequently sexually active, are ill-informed about the availability and use of contraceptives. (See Chapter V for a description of fertility-related characteristics of STEP youth in Year Three.) The LSO component

Table IV.7

MEAN HOURS (AND PERCENTAGE) OF PARTICIPATION IN CLASSROOM AND WORK EXPERIENCE COMPONENTS[a]

	Returning Cohort II Youth			Cohort III Youth		
	Classroom Hours (%)	Work Hours (%)		Classroom Hours (%)	Work Hours (%)	
Site	Treatments	Treatments	Controls	Treatments	Treatments	Controls
All Sites	99(85%)	94(75%)	206(88%)	100(85%)	99(79%)	197(82%)
Boston	109(82%)	66(76%)	N.A.	109(83%)	72(81%)	164(87%)
Fresno	101(88%)	150(81%)	N.A.	99(86%)	153(83%)	255(85%)
Portland	93(82%)	75(75%)	N.A.	93(83%)	82(82%)	167(80%)
San Diego	103(88%)	90(88%)	200(91%)	104(89%)	93(91%)	201(91%)
Seattle	91(83%)	88(70%)	212(86%)	95(87%)	94(75%)	200(86%)

N.A. = not applicable (Control youth were not offered a second summer of work experience in these sites.)

[a] An analysis of the determinants of program participation is presented in Appendix A. We found that the best predictor of classroom attendance is a youth's attendance pattern during the preceding school year.

also covers other social concerns facing youth, such as career planning, substance abuse and coping with stress.

The Curriculum

The LSO curriculum integrates teens' preparation for future employment with planning for their future families. It focuses on two critical areas in which youth must make decisions: the world of work and issues of sexual and reproductive development, feelings and behaviors.

There are two volumes of the LSO curriculum, one for each summer of STEP participation. Although the aims and themes of the two volumes are identical, their emphases vary in order to respond to the different age levels of the youth receiving each volume.

Volume I, used with first-summer youth, provides an introduction to the issues and situations that a teenager is likely to face and suggests mechanisms for making responsible decisions. Topics covered in the first volume include values clarification, decision-making, sex-role stereotyping, the physiology of sex, use of contraception, and sexual relationships and responsibility. An informational visit to a comprehensive clinic or other health facility is also part of the first-summer curriculum.

Volume II reinforces the themes and concepts of Volume I, and begins to focus on work attitudes and behavior, career exploration, effective communication and health maintenance. Volume II strongly addresses the risks of sexually transmitted diseases, substance abuse and stress.

Schedule

Youth from both cohorts participate in LSO classes for 18 to 20 hours over the course of a summer. Typically, LSO classes are offered twice weekly in 90-minute sessions during usual remediation hours. In Boston, however, youth had to remain at the remediation site after lunch for LSO instruction on two afternoons each week. All sites, including Boston, plan to offer LSO during regular remediation hours in the final summer of the demonstration.

Teacher Training and Class Preparation

Prior to the start of the summer program, life skills instructors from all sites attended a two-day comprehensive training workshop delivered by the authors of the curriculum. One session was held at a West Coast location and was attended by teachers from the four West Coast sites; a separate session was held for Boston teachers. Training included an overview of the demonstration, a review of curriculum content, an analysis of instructional techniques, a discussion of classroom management issues and a series of role-plays and other sensitizing activities. In Year Three, an in-depth discussion of AIDS was also part of the training.

LSO instructors assessed training very positively. Ninety-two percent of those who attended training rated the sessions good or excellent. However, nearly two-thirds of the teachers felt that the amount of training provided was insufficient. Teachers reported that they needed more time to review LSO sessions, instructional techniques, program philosophy and classroom management issues.

LSO instructors received less in-service training than remediation teachers. P/PV's consultants, who visited each site at a mid-program point, did provide some technical assistance, and local efforts to provide technical assistance were also made. Three-quarters of the instructors reported receiving some mid-program training.

Fifty-seven percent of the teachers reported receiving in-service training from a P/PV consultant, while 43 percent reported receiving training provided by local staff. Although 90 percent of the LSO instructors rated the quality of in-service training as good or excellent, more than half of the instructors felt that the amount of training provided was insufficient.

Preparation for classes outside of class hours was also required of LSO instructors. On average, teachers across sites reported spending 4.9 hours each week on LSO preparation. Site differences in the amount of time spent preparing for classes was evident in the teachers' reports. In Boston, teachers reported spending an average of 3.3 hours each week in preparation; teachers in Fresno and Portland reported spending an average 4.3 hours weekly; San Diego teachers reported an average of 7.8 hours weekly; and teachers in Seattle reported spending an average of 7 hours each week.

Staffing Patterns and Teacher Characteristics

To facilitate the integration of the LSO component with the remediation program, the role of lead teacher was expanded in 1987 to include oversight of the LSO component. This role expansion involved becoming familiar with the LSO curriculum, conducting classroom observations, ensuring that materials were available, and providing instructional support to LSO teachers by helping them with classroom management problems, student discipline issues and instructional strategies.

To ease them into this new role, lead teachers were invited to participate in LSO training. Since the decision to expand their role came late in the planning phase for the summer, however, only three lead teachers were able to attend the training.

The degree to which lead teachers assumed the LSO oversight role varied by site. In Boston, program operators believed that oversight of LSO was an inappropriate task for the lead teacher. Instead, another staff person was designated as LSO coordinator and was given responsibility for overseeing the component. In other sites, oversight of LSO by lead teachers was uneven. Some lead teachers felt uncomfortable providing technical assistance because they were unable to attend the training sessions. Lack of familiarity with the material and the general subject matter made others less effective. Despite these implementation problems, the inclusion of LSO instructors in general staff meetings and the designation of a lead person as an LSO coordinator fostered better integration of program components. However, it may take several summers before the role of the lead teacher in the LSO component is fully clarified and effective.

Twenty-eight instructors staffed the LSO component across the five sites. Boston had the most instructors--11. Nine were counselors and two were family planning educators from ABCD. In the other sites, LSO instructors were hired through the school districts. Four instructors were hired in Fresno, Portland and Seattle, and five were hired in San Diego. (See Table IV.8 for selected characteristics.)

Across sites, 71 percent of LSO instructors were female; however, gender balance was achieved in the Fresno and Portland programs. Half of those hired to deliver LSO instruction were black, 11 were white, two were Hispanic and one was from an unspecified racial/ethnic background. Fifteen LSO teachers (more than half) had taught in STEP during a previous summer. Ten teachers were teaching in the program for a second summer, while five were teaching in their third.

Unlike the remediation teachers, some of whom taught both cohorts of youth, LSO instructors were assigned exclusively to one cohort or the other. Sixteen instructors taught Cohort III youth and 12 taught Cohort II youth.

Average teaching experience for LSO instructors ranged from seven years in Boston to 20 years in San Diego; reported experience covered the full range of grade levels from elementary to high school. Nineteen of the 28 instructors previously taught health/sex education courses. Thirteen instructors had a master's level degree, 10 had a bachelor's degree, two had a doctorate, and one had an associate's level degree. The remaining respondent's level of education was not specified.

Only 10 of the 28 LSO instructors had access to an instructional assistant. Aides were available to all LSO instructors in the Fresno and Seattle sites, while two instructors in Boston shared an aide with another teacher(s). In LSO, aides directed small

Table IV.8

LSO TEACHER CHARACTERISTICS BY SITE

	All Sites	Boston	Fresno	Portland	San Diego	Seattle
Gender						
Male	8(29%)	3(27%)	2(50%)	2(50%)	1(20%)	0
Female	20(71%)	8(73%)	2(50%)	2(50%)	4(80%)	4(100%)
Race/Ethnicity						
Black	14(50%)	6(55%)	1(25%)	2(50%)	3(60%)	2(50%)
Hispanic	2(7%)	1(9%)	1(25%)	0	0	0
White	11(39%)	4(36%)	2(50%)	2(50%)	2(40%)	1(25%)
Other	1(4%)	0	0	0	0	1(25%)
Program History						
1st-Year Teacher	13(46%)	5(46%)	3(75%)	0	2(40%)	3(75%)
2nd-Year Teacher	10(36%)	4(36%)	1(25%)	4(100%)	1(20%)	0
3rd-Year Teacher	5(18%)	2(18%)	0	0	2(40%)	1(25%)
Cohort Assignment						
Cohort II	12(43%)	4(36%)	2(50%)	2(50%)	2(40%)	2(50%)
Cohort III	16(57%)	7(64%)	2(50%)	2(50%)	3(60%)	2(50%)
Teaching Experience						
Mean Years of Experience	10	7	8	12	20	10
Sample Size	28	11	4	4	5	4

group activities and performed clerical tasks, such as photocopying and distributing materials. All instructors who had aides reported that the aides were useful.

Observations and Assessments

In general, instructors felt that the content of the LSO curriculum was appropriate for the population. Twenty-four of the 28 teachers made this assessment, and 23 believed that new information was being provided to youth through the curriculum.

Curriculum strengths cited by teachers included the use of real-life situations; the nature and variety of activities; the provision of straightforward, pertinent facts; and the use of interactive instructional methods that promote critical thinking skills. Teachers further commented that the curriculum was well-written and included good instructions, that good audiovisual materials were selected and that the class provided a secure and comfortable environment in which students were encouraged to discuss sensitive issues and ask questions.

Eighty-one percent of LSO teachers reported that the clinic visit was well-planned and useful. This represents an improvement from the previous year, when only about half the teachers rated the visits positively. In general, teachers who did not rate the visit positively reported that the visit failed because of a poor choice of speakers at the facilities, disorganized facilities and/or a lack of effort by facilities to make information relevant and interesting for STEP youth. Over the course of the demonstration, we have learned that willingness of clinic staff to host visits cannot be assumed. It is important to do careful advance planning; a personal visit to the clinic by the LSO coordinator or other staff member and a pre-visit meeting with clinic staff are also useful.

Areas of the curriculum that instructors assessed as needing improvement were employment and career development, health and nutrition, and the risks associated with substance abuse and AIDS.

As with the remediation component, structured observations of life skills classes were conducted. Seventy-six life skills sessions were observed across the five sites. On average, 16 students were present in the observed classes. In addition to the instructors, an aide was present for 30 percent of the observations (22 classes).

A wide range of instructional approaches were used in LSO classes. Activities involving the full group of students were reported in 34 percent of classroom observations; small group activities were reported in 27 percent of classes; direct presentations by the instructor in 14 percent of the classes;

discussions/debates were reported in 10 percent of classes; and films, role-plays, and/or outside speakers were reported in 14 percent of the observed classes. Observers reported that LSO materials were followed in three-fourths of the observations, while some deviation from the curriculum occurred the remainder of the time.

Student response to the classes was very positive. In 75 percent of the observations, observers described students as interested in the lesson; they were characterized as bored in only 12 percent of the observed classes. Furthermore, in 82 percent of the observed classes, observers reported that most or all students were engaged in the learning activities.

Environmental conditions in the classrooms (both remediation and LSO), such as lighting and room temperature were reported as satisfactory in all sites. Staff in the Fresno and San Diego programs decorated their classrooms with STEP slogans and other posters to further enhance the educational environment.

The quality of instruction also got high ratings. Instruction was described as good or excellent in 70 percent of observed classes, and 92 percent of the time instructors were said to be knowledgeable about the topics.

LSO Summary

As was the case in previous demonstration years, the LSO component was enthusiastically received by both teachers and students in Year Three, and the component was successfully implemented. The Volume II curriculum, used with second-summer youth, continued to be the weaker of the two volumes. Information covered in that curriculum on career issues, substance abuse, and health and nutrition was frequently cited as needing further work. Additional modifications to this volume are being made for the final demonstration year.

Successful implementation of the LSO component was helped by a high percentage of returning teachers. More than half of the LSO instructors had taught in STEP in at least one prior summer.

The role of the lead teacher was expanded in the third year of the demonstration to include LSO coordination. This role was unevenly assumed and implemented across the five demonstration sites. The addition of overall LSO responsibility to the lead teacher role came late in the planning stage for the summer, and several lead teachers were unable to attend LSO training. As a result, many were uncomfortable with the responsibility. Despite this uneven implementation, the designation of a lead person for the component is still believed to be essential. Other efforts to improve the integration of the remediation and LSO components, such as holding joint staff meetings and team-teaching efforts in Fresno, were generally successful.

THE WORK EXPERIENCE COMPONENT

The most familiar aspect of the STEP model is traditional summer work experience. Treatment youth worked five afternoons per week (three in Boston)--after attending classes in the morning--in jobs obtained through the regular SYETP program. Work assignments for treatment youth usually spanned three to four hours daily. Control youth received the same benefit they would ordinarily get through SYETP--a full-time summer job.

Although job development and placement are routine functions of SYETP agencies, STEP presented a new challenge at the beginning of the demonstration. First, service to 14- and 15-year-olds was not typical. Second, employers were resistant to using part-time workers during the summer months. Convincing employers to hire younger than usual workers, as well as part-time workers, frequently required creative negotiating.

Over time, STEP staff have become savvy negotiators and employers have become less resistant to hiring younger youth and part-time summer help. One strategy used by sites to secure employment for STEP treatment youth was bargaining for the placement of a specific number of part-time youth at a job site in exchange for placing a specific number of full-time workers at the same site. Employers were also swayed by explanations of the STEP concept--many saw the possibility that integration of the classroom and work components would produce better workers. Still others were employers faced with the choice of no summer help versus part-time and younger summer help.

Job Development and Program Integration

Job development procedures were fairly similar across sites. In reality, actual job development was limited in 1987, since most employers involved with SYETP and STEP are repeat employers who have hired youth as summer workers for a number of years.

In general, the job development process involved application or proposal submission by employers to SYETP agencies detailing the number of youth needed for specific positions. Boston was an exception; since treatment youth worked at the universities where they received remediation, various university departments provided work projects for these youth. Some of these had been used as work sponsors in previous summers, but development of new ones was also required. Control jobs in all five sites were developed through regular SYETP channels. In some sites, STEP youth were given priority for the limited SYETP slots.

The "job development" process began as early as January in Fresno and as late as May for treatment youth in Boston. The other sites commenced job development activities in March or April. Job placement typically was complete by the end of June or early July. All sites used a questionnaire or other inventory mechanism to determine youth preferences and their suitability for jobs. Cohort II youth returning for a second summer were given first preference of selection and in several sites, they had an opportunity to participate in job interviews. Except in Boston, Cohort III youth were unable to participate in job interviews with employers because group assignments were not revealed until after pretesting.[26]

To foster integration of the work component with the classroom components of the program, several strategies are used. First, the content of some modules (primarily Adjusting to the World of Work and Your Summer Job) focuses on workplace and career issues. Employability skills are also a special focus of the Volume II curriculum for LSO. Finally, in some sites JTPA agency staff go to remediation sites to pick up timesheets and are able to discuss individual students with teachers during these visits. Efforts at better integration have been intensified each year and both the content of materials used in the curriculum and communication between the worksite and school site staffs have improved. In Year Three, the San Diego site added the position of career technician to its staff; a "career tech" was placed at each remediation site and was given responsibility for keeping attendance records on a daily basis. By centralizing this function and reducing the number of remediation subsites to two, San Diego improved communication between the school and worksites and made enforcement of the no school/no work sanction for treatments easier. The "career tech" called employers in the morning and relayed information on any youth who should not be allowed to work.

Other sites also attempted to upgrade component integration. In Seattle, instructional assistants were used as worksite supervisors for Cohort III group projects, and youth kept work journals as part of the remediation experience. In Boston, where work and class experience were colocated, communication was easy; LSO instructors in Boston also served as worksite liaisons and were placed in charge of attendance.

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[26] In Boston, the first week is spent on a module titled Your Summer Job. As part of this instruction, treatment youth are interviewed by their potential employer for employment that begins later in the first week of the program.

Job Placement

For the most part, the jobs secured for each group were typical summer youth employment positions. Positions as clerical assistants, maintenance workers, recreation aides and day care assistants for public sector agencies were frequently held by STEP treatments and controls. However, there were some differences in the nature of the jobs assigned to treatment and control youth in several sites.

In addition to typical SYETP positions, a number of special projects have been available to STEP youth over the course of the demonstration and a number of improved work assignments have been obtained each year. In Boston, for example, treatment youth worked at university campus jobs, which were unlikely to be available in the traditional program. In Seattle, agencies submitted proposals for special community projects to which STEP youth were assigned. The Naval facility in San Diego also provided a unique work opportunity for STEP youth.[27]

Efforts to secure upgraded job assignments for returning youth were made in all sites, since returning youth are promised better jobs as part of the reenrollment strategy. Sites were fairly successful in providing much-improved job assignments to returning youth. Only 23 percent of the Cohort II returnees (23% percent of the treatments and 24% of controls) reported working in a custodial position, compared with 40 percent of the first-summer youth in Cohort III. Custodial positions are the least popular with youth. Also, 39 percent of the treatment returnees were placed in a clerical position and 23 percent worked in jobs involving children, including recreation, tutoring or child care positions. Table IV.9 describes types of jobs obtained by site and treatment status.

Forty-four percent of returning treatment youth reported in questionnaires completed at the end of the summer that they preferred the second summer's job over the first summer's job. Thirty-two percent rated their two STEP jobs equally, while only 24 percent preferred the first summer's job. Among returning controls in San Diego and Seattle, 39 percent preferred the second job over the first.

In the two sites serving Cohort II controls, differences in the types of jobs obtained were found between treatments and controls. In Seattle, the distribution of job assignments was significantly different for treatment and control youth. Clerical positions were disproportionately assigned to treatment

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[27] Unfortunately, transportation constraints limited the Naval facility assignments to control youth only.

Table IV.9

JOB CLASSIFICATIONS FOR COHORT II AND III YOUTH BY SITE

	All Sites		Boston		Fresno		Portland		San Diego		Seattle	
Job Type	Treatment	Control	T	C	T	C	T	C	T	C	T	C
Cohort II												
									##		##	
Custodial/ Maintenance	23%	24%	11%	N.A.	32%	N.A.	20%	N.A.	35%	20%	14%	27%
Recreation/Work with Children	23%	28%	5%	N.A.	29%	N.A.	30%	N.A.	27%	33%	21%	26%
Clerical	39%	29%	68%	N.A.	31%	N.A.	30%	N.A.	29%	31%	43%	26%
Food Service	4%	6%	8%	N.A.	0	N.A.	7%	N.A.	3%	4%	4%	8%
Other	11%	13%	8%	N.A.	8%	N.A.	13%	N.A.	6%	12%	18%	13%
Cohort III												
	###		###				###		###		###	
Custodial/ Maintenance	41%	40%	16%	18%	57%	65%	59%	55%	43%	32%	30%	25%
Recreation/Work with Children	17%	28%	16%	57%	19%	12%	12%	18%	23%	17%	17%	40%
Clerical	25%	14%	51%	9%	16%	12%	20%	8%	20%	31%	16%	8%
Food Service	1%	5%	1%	3%	0	1%	2%	9%	0	6%	0	7%
Other	16%	13%	16%	13%	9%	10%	7%	10%	14%	15%	37%	20%

N.A. = Not applicable (Cohort II control youth were not offered a second summer of work experience in these sites.

###The distribution of job types was significantly different for treatments and controls at a 0.01 level.
##The distribution of job types was significantly different for treatments and controls at a 0.05 level.

youth, while a substantially higher number of control youth were given custodial positions. In San Diego, however, treatment youth were more likely than controls to hold a custodial position.

There were also differences in the types of jobs obtained by first-year treatments and controls from Cohort III. While treatments and controls were about equally represented in custodial positions overall, they were differentially assigned to recreation and clerical positions. Control youth were more likely to be assigned to recreation or food service positions, while treatments were more likely to be assigned to clerical positions.

Differences in job assignments for treatments and controls were found in all sites except Fresno. In Boston, treatment and control youth were disproportionately assigned to jobs involving recreation and clerical work; in Portland, treatments and controls showed significant differences with respect to assignments in the clerical and food service areas; treatment youth in San Diego were more likely to have custodial jobs and less likely to be placed in clerical or food service positions; and treatment and control youth in Seattle were disproportionately assigned to jobs in the recreation, clerical, food service and other categories.

Attendance Rates

In general, treatment youth spent at least half their total program hours on work projects. Thus, the sum of work hours and classroom hours should be roughly equivalent to the total hours offered to control youth.[28] Treatment youth were offered 87 hours of work in Boston, 95.5 hours in Portland, 96 hours in San Diego and 125 hours in Seattle. In Fresno, treatment youth were offered as many as 233.5 hours.[29] Control youth were offered hours ranging from 188 in Boston to 392 in Fresno.

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[28] Differences in hours between the two groups, however, were quite large in Boston and Fresno. Control youth in Fresno were able to participate for 43 more hours than were treatments, while treatment youth in Boston had 31 more program hours than controls.

[29] Excess appropriations for the summer program made it possible for Fresno to offer youth additional hours--184.5 work hours were originally planned for treatment youth and 300 for controls. These extra hours enabled youth to work beyond the end date of the program and the collection of payroll data for the demonstration. Therefore, the rates of participation shown are based on the highest possible maximum work hours, and are somewhat distorted.

Table IV.7 revealed no differences in the overall participation rates of Cohort III treatments and controls in 1987. Across sites, treatments participated for an average of 199 hours and controls participated for an average of 197 hours; this represents 82 percent of available time for both groups.

In San Diego and Seattle, program participation was lower among Cohort II treatment youth than for Cohort II controls. While Cohort II control youth in San Diego and Seattle participated for an average of 91 and 87 percent of available hours, respectively, treatment youth had overall participation rates of 88 percent and 77 percent, respectively. Furthermore, participation in work experience activities was lower for Cohort II treatment youth than Cohort III treatment and control youth in every site.

Observations and Assessments

Treatment youth from Cohort III liked their jobs more than controls did. Seventy-one of Cohort III treatment youth indicated on summer-end questionnaires that they liked their jobs either a lot or very much, compared with 51 percent of controls. While only 11 percent of the treatments reported not liking their jobs, one-fourth of the controls did so.

Treatment and control youth from Cohort II gave roughly equivalent assessments of their jobs--52 percent of the treatments and 56 percent of the controls liked their assignments, while 18 percent of both groups did not.

For treatment youth in both cohorts, the most important program benefit was judged to be learning to make decisions. This was followed by the opportunity to earn money and learning a specific work skill. For controls in both cohorts, learning a specific skill and general knowledge of work were cited as the most important program benefits.

Along with these assessments from program youth, we examined 74 structured observations completed by P/PV's site coordinators. Of the 74 worksites observed, 24 were private not-for-profit organizations, 17 were government agencies, 16 were community-based organizations, six were school districts, and 11 were private, for-profit firms or other unspecified agencies.

Worksite supervisors were interviewed as part of the observation process. Supervisors at 29 sites (41%) indicated that their organization had previously hired STEP youth; those at 45 sites (70%) had previously hired regular SYETP youth. Just under one-third of the supervisors had supervised STEP youth prior to the 1987 summer and two-thirds had experience supervising 14- and 15-year-olds prior to STEP. Fifty-three percent of those interviewed supervised treatment youth, 24 percent supervised controls and 23 percent supervised both groups of youth at their worksite.

Overall, supervisors gave the job performance of both treatment and control youth high marks. Eighty-seven percent of those supervisors interviewed rated treatment youth job performance as good or excellent; 88 percent gave control youth a similar rating. Eleven percent of the treatments received a fair rating and 2 percent were rated as poor, while controls received fair marks in 9 percent and poor ratings in 3 percent of the observations. The two groups were also very evenly rated by their supervisors with respect to work attitudes. Eighty-three percent of both treatments and controls received good or excellent ratings on their attitude. When asked for areas in which youth most needed improvement, STEP supervisors most frequently mentioned initiative. Failure to call in if late or absent was the second most-cited shortcoming, followed by failure to ask for clarification on unclear assignments, lack of punctuality and improper dress habits. Ninety-two percent of those interviewed said they would recommend hiring another STEP youth next summer.

THE SCHOOL-YEAR SUPPORT (SYS) COMPONENT

During the school year between the first and second summers of participation, treatment youth are offered a variety of support services through the integral but voluntary[30] SYS component. SYS services are primarily designed to help youth achieve academic and social success during the school year, but they are also meant to encourage youth to return to the summer program for a second summer.

SYS is the least prescriptive of the four STEP components. While guidelines for basic operational requirements exist, they allow sites a good deal of latitude in designing the component. For example, P/PV specifies the number of contacts required for each youth, but the nature of those contacts is at the discretion of the site. Therefore, the provision of SYS services can be very individualized. In general, services are provided directly or through referrals and include personal and academic counseling, tutoring and social activities. However, a host of other services, including intervention in legal and family situations, are not uncommon.

One service that all sites identified as a serious need of STEP youth is tutoring. All sites made efforts to provide tutors for youth through existing school district programs or SYS grant funds, but all found such services difficult to coordinate. First, many students did not admit to needing help; even when they did, many failed to keep tutoring appointments. Also, services available through the school district were not always appropriate or convenient for STEP youth. Finally, transportation often proved a barrier to getting youth and tutors together.

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[30] Student participation in SYS is voluntary, but all sites are required to make SYS services available.

Structure and Guidelines

P/PV's guidelines for SYS require that a staff consisting of at least a half-time coordinator and several advocates or counselors be assembled for SYS. Advocates are required to work a minimum of 15 hours each week on caseloads of between 15 and 20 students. P/PV also requires that advocates contact youth at least twice monthly on an individual[31] or small group basis. Also required are quarterly contacts in large group settings. Parents must also be kept informed about SYS activities. Finally, sites are encouraged to secure the support of the local school system in planning and managing SYS activities. Schools can designate school staff members to monitor attendance and performance of STEP youth; ensure that tutorial, health and other counseling services are provided to STEP youth; and/or provide space and other in-kind support for SYS activities.

Responsibility for SYS activities can be assumed by the lead agency for the demonstration or any other participating agency. In SY 1986-87, the lead agencies in Boston and Portland were directly responsible for implementing SYS. In Fresno and San Diego, SYS was managed and implemented by the participating school district. P/PV directly hired and supervised the coordinator for the Seattle program and assumed the major responsibility for SYS, in cooperation with the lead agency. The Seattle agencies were unable to submit a proposal for the component that met P/PV's guidelines, but were amenable to working with a P/PV-managed SYS staff.

School Year (SY) 1986-87

During the 1986-87 school year, members of the Cohort II treatment group were eligible to receive SYS services. This was the second opportunity in the demonstration to implement SYS and for the first time, all sites did so. San Diego did not operate an SYS component in the previous school year. The 1985-86 SYS program was identified as an area needing comprehensive improvements. To that end, P/PV assumed a more intensive oversight role of SYS in all sites during the 1986-87 school year.

Site Descriptions

Boston. SYS in Boston got into full swing very late because the full-time SYS coordinator was not hired until late November. However, SYS activities were held earlier in the school year, with the program initially managed by the local STEP advisory committee. Boston's initial plan for SYS was to hire three counselors who had worked with students during the summer and give them responsibility for caseloads of 25 students each. In addition, 75 students were to be placed in other district dropout prevention programs. Unfortunately, the plan to link STEP youth with other dropout prevention programs was not implemented because the main program to which STEP youth were to be referred was not implemented in time. This resulted in larger caseloads for program staff and the late hiring of an additional counselor. In addition, counselors were hired to work 15 hours weekly, yet worked an average of only 10 hours weekly.

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[31] Individual contacts can be made by telephone; however, at least one monthly contact should be done face-to-face.

As a result of the staffing and programming difficulties, youth in Boston received less than the mandated twice-monthly contact. On average, youth were contacted 1.28 times per month over the school year. Eighty-eight percent of Boston's caseload received at least one contact during the year.

Boston held a variety of group activities for youth, including two citywide events to which all students were invited and two events at each university for youth who had attended summer remediation on that campus.

Fresno. A half-time coordinator and five 15-hour-per-week counselors were hired to staff the SYS component in Fresno. Counselors were graduate students in counseling, social work, psychology, education or related fields who were secured from a local university. These students received field credits and monetary payment for their work with STEP youth and were responsible for caseloads of approximately 25 youth. Youth in Fresno were contacted an average of 3.57 times per month, the highest contact rate of the five sites. Table IV.10 shows that Fresno also had the highest rate of contact with their caseload, with 97 percent of youth receiving at least one contact.

A host of small and large group activities were held for youth in Fresno, including field trips to local businesses and universities and a variety of social events. Fresno also instituted a system of incentives for youth that were tied to grade improvements and SYS participation. A parent advisory group was also formed. These high contact rates illustrate the high level of commitment to SYS in Fresno. Site staff were able to garner the support of several community agencies and businesses; many became involved by hosting trips and providing donations for the incentive program and other activities.

Portland. The level of commitment to SYS in Portland was also very high. In addition to the P/PV grant, the Portland PIC provided funding for implementation of SYS. (P/PV funding accounted

Table IV.10

SUMMARY OF SCHOOL-YEAR SUPPORT CONTACTS BY SITE

	All Sites	Boston	Fresno	Portland	San Diego	Seattle
Average Number of Monthly Contacts in SY 1986-87	2.03	1.28	3.57	2.17	1.52	1.62
Number (Percentage) of Youth With at Least One Contact	1,412 (90%)	288 (88%)	301 (97%)	317 (95%)	247 (83%)	259 (85%)
Number (Percentage) of Youth Never Contacted	162 (10%)	41 (12%)	9 (3%)	17 (5%)	50 (17%)	45 (15%)
Sample Size	1,574	329	310	334	297	304

for less than half of Portland's SYS budget.[32]) As a result, Portland's program was staffed by a full-time, year-round coordinator who also coordinated the summer components, and four advocates who each worked 30 hours per week, most of whom also worked in the summer program. This staffing pattern provided good program continuity and allowed staff to devote more time to following up on youth as well as actually providing services.

Portland provided youth with a variety of academic and counseling services and offered incentives for participation linked to academic achievement. A system in which a school staff person in each school was identified to advocate for STEP youth was also employed by the Portland program. This program feature was an uneven success because it depended largely on the school staff person identified. On average, youth in Portland were contacted 2.17 times per month over the school year; 95 percent of Portland's caseload received at least one contact.

San Diego. The 1986-87 school year was the first year in which SYS services were provided in San Diego. Managed by the Social Concerns Unit of the Health Services Department of the school district, the component was staffed by a part-time coordinator and two counselors who had been with STEP as LSO instructors since the beginning of the demonstration. Excellent counselors and program continuity proved the program's greatest strengths. Counselors travelled to different schools in the district as part of their regular assignments and arranged to see STEP youth during these visits. The counseling provided to youth in the San Diego program was a noteworthy aspect; however, the high caseloads were perhaps the program's biggest weakness. To combat this, San Diego instituted the "favorite teacher concept," which P/PV adapted for demonstration-wide use in SY 1987-88. Students identified a favorite teacher in their school and the SYS counselor spoke to that teacher about the youth, soliciting their support in the youth's STEP experience.

Youth in San Diego received an average of 1.52 contacts per month during the school year. San Diego had the highest percentage of youth with whom no contacts were made--17 percent or 50 youth.

Seattle. As noted earlier, the SYS component in Seattle was coordinated by a P/PV-hired staff person and it was difficult to get momentum going without local institutional backing and ownership. This was, perhaps, the biggest drawback in the Seattle program during the 1986-87 school year. (For SY 1987-88, a stronger linkage with the Seattle Public Schools has been established, thereby substantially diminishing this problem.)

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[32] P/PV's school-year support grant was the primary and, in some cases, only funding for SYS at the other sites, where local contributions were limited to in-kind contributions, such as staff time, space or tickets to events.

In addition to the P/PV-hired coordinator, the component in Seattle was staffed by three mentors who worked 15 hours per week and a tutor and an outreach worker for Indochinese students, each of whom worked 20 hours each month. Activities held for the SYS component in Seattle included visits to local universities, a career workshop, and a variety of social and sporting events. Youth in the Seattle program averaged 1.62 monthly contacts from advocates over the school year. A high percentage of youth (15%) had no contact with the SYS component.

Group Activities and Program Exposure

Sites reported that SYS activities for both large and small groups tend to be attended by the same core group of youth. Large group activities generally attracted fewer participants than expected and led many sites to report that resources would be better spent on additional small group events. Barriers to participation, such as conflicting after-school activities or jobs, transportation, and the lack of student recognition that such services as tutoring are needed are often reasons for this lack of participation. To eliminate some of these barriers, advocates or counselors often arranged to meet youth at school during a study period or at lunch break. Some sites have also instituted a series of incentives to enhance the image of SYS among STEP youth. Schemes that included awards tied to improvement in grades, attendance and/or participation in SYS activities have been employed. STEP newsletters were published in Fresno, Portland and San Diego as a means of encouraging youth to participate.

As shown in Table IV.10, the number of successful contacts with youth varied by site. The highest rates of contact were found in the Fresno and Portland programs; Fresno exceeded the required minimum contacts of two per month by more than 1.5 contacts, while Portland met the required minimum. Contact rates also varied by month. In three sites, the lowest rates of contact were seen in the early months of the school year--September or October. This was true in Boston, Fresno and Seattle. However, September was the month of most frequent contact in Portland, as was October in San Diego. The high contact month for Boston, Fresno and Seattle was March or April, the months when reenrollment activities commenced.

Sites reported a high degree of difficulty in contacting youth. For example, the time of day during which a youth could be successfully contacted did not always coincide with the schedule of the assigned counselor and schedule modifications had to be made; some youth could be contacted at schools while others could not.

Substantial numbers of disconnected phones further exacerbated the problem.

School-Year Support Summary

Overall, site visits made by P/PV's central staff and monthly reports submitted by SYS coordinators indicate that SYS was improved during the 1986-87 school year. Unlike the first school year, all sites provided an SYS component for youth. Despite more intensive programs, however, exposure to SYS was not as high as expected in most sites. All sites had particular program strengths and weaknesses, but many programs suffered from the same difficulties.

Tutoring services, which all sites identified as an area of great need, were particularly difficult to arrange and manage. The provision of tutoring services was made particularly difficult because of financial constraints. Sites had to depend on existing tutoring resources in the community, which were not always appropriate or convenient to the needs of STEP youth. In addition, students who admitted a need for tutoring often failed to keep appointments. Transportation was also a serious barrier to arranging tutoring. Many STEP students are bused to schools far away from their homes and can't remain for after-school tutoring.

For the 1987-88 school year, P/PV took the "best elements" of several programs and modified program requirements. Modifications included the institution of the favorite teacher concept, the publication of newsletters at all sites, the provision of tutoring services by SYS, and the development of individual year plans for each student. In addition, P/PV continued its increased oversight role, maintaining intensive monitoring and providing technical assistance.

SUMMARY OF PROGRAM IMPLEMENTATION

Despite a late decision to serve a third cohort of STEP youth, the various components of the STEP model were implemented with a high degree of success during the third year of the demonstration. Sites are familiar with the many elements of the model and requirements for the research. Despite this familiarity, however, some problems have persisted year after year. One such problem is the recruitment and reenrollment of STEP youth. The need for early outreach and recruitment efforts seen in previous summers was echoed by the experiences with Cohort III. Sites with expanding economies, in particular, must develop creative strategies to lure youth into and back to the program. The granting of academic rather than elective credits for participation is one possible incentive.

The remediation component had its smoothest implementation to date. A large number of returning teachers aided in this success. Sites are familiar with and supportive of the instructional approaches utilized in STEP. Students also responded positively to the program. Ninety percent of treatments and 89 percent of the controls from Cohort III reported they would recommend the program to a friend. Seventy-three percent of treatments and 70 percent of controls said they would like to return to the program next summer; 18 percent of treatments and 21 percent of controls were unsure about returning and 9 percent of both groups were negative. Of returning Cohort II youth, 77 percent said they would recommend the program to a friend.

Life skills continued to be a popular program element in the sites. Volume I, used with first-year students, was very well received and implemented in Year Three. Volume II, though well received, was seen as needing improvement. Teachers reported that the curriculum needed strengthening in the areas of drug and substance abuse, health and nutrition, AIDS and career issues. These areas are the focus of revisions for the final demonstration year.

Work experience, the most familiar of the program elements, was also well-implemented. Returning students received better jobs for the most part and supervisors were pleased with the attitudes and work performance of STEP youth. Worksite developers for STEP youth--part time as well as full time--no longer met the levels of resistance seen in the first two demonstration years.

Finally, the 1986-87 SYS programs experienced more success across sites than during the previous school year. Participation in group activities continued to be disappointing, however, and in three sites, rates of exposure to SYS through contacts were still lower than the mandated two per month. These areas were the focus of increased oversight during the 1987-88 program.

V. PARTICIPANT CHARACTERISTICS AT BASELINE

This chapter presents characteristics of the two groups of youth who participated in STEP during summer 1987. First, we describe the characteristics of the third and final group of demonstration enrollees, Cohort III.[33] Next, Cohort II youth who returned for their second summer of service are profiled. The latter group includes treatment returnees in all five sites and control returnees from the San Diego and Seattle sites. These youth are described separately. Finally, this chapter describes Cohort I and II youth who participated in the first wave of follow-up surveys. These surveys were conducted between October 1987 and January 1988.

Descriptions of all youth are based on data collected at baseline. Data for Cohort III were collected at the beginning of the 1987 summer, while data for Cohort II returnees were collected in 1986 at the beginning of their first summer. Using program baseline data for returnees allows program operators to project which youth can be expected to return to a program like STEP for a second summer.

THE COHORT III SAMPLE

In 1987, a sample of 1,591 youth was randomized to either STEP treatment or control groups.[34] Of this initial sample, 21 percent (340 youth) either did not participate in the program at all, did not complete MAT pretests or posttests, or did not complete key questionnaire items. The remaining 1,251 youth constitute the learning analysis sample[35] and form the basis for the descriptions presented in this chapter.

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[33] For all characteristics that are examined, the similarity of the treatment and control groups are tested. Unless otherwise stated, there are no statistically significant differences between treatments and controls. Consistent with the aggregation of data for Cohorts II and III in Chapter VII, comparisons to previous cohorts are restricted to members of the second cohort. Such comparisons are made only when differences are substantial or reach statistical significance.

[34] There were 1,594 youth randomized in Cohort I (1985) and 1,635 youth randomized in Cohort II (1986).

Accurate estimates of program impacts depend on the difference in patterns of attrition that occur between one sample and the other. Therefore, statistical tests to determine the nature of attrition from the randomized sample to the learning analysis sample were conducted. Cohort III's attrition rate from the randomized to the learning analysis sample is very similar to that of Cohort II--about a 22 percent reduction. Unlike last year, however, statistical tests indicate that attrition differed for treatment and control groups. Controlling for other factors, Cohort III treatment youth were 7 percent more likely than controls to be part of the learning analysis sample.

The key demographic variables--gender, age and race/ethnicity--are presented for both the randomized and learning analysis samples of Cohort III in Table V.1. Subsequent descriptions in this chapter refer exclusively to the learning analysis sample.

Demographic and Economic Characteristics

The Cohort III population is very similar to the previous two cohorts of STEP youth. Like earlier cohorts, Cohort III was a group of youth at an age (14 or 15) when employment opportunities are limited, who were mostly minority and poor, and who had substandard basic skills. The demographic, economic, educational and sexuality characteristics of these youth, discussed below confirm the need for an intervention like STEP in their lives.

Age

Overall, as shown in Table V.2, more 14-year-olds (58% of the sample) were enrolled in Cohort III than 15-year-olds. Younger participants accounted for more than 60 percent of the samples in Boston, Portland and San Diego. In Fresno, however, more than half of the youth (52%) were at least 15 at entry, and both ages were equally represented in the Seattle program.

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[35] The learning analysis sample includes all Cohort III youth having pre- and posttest MAT scores and valid responses to all variables included in the multivariate learning analysis. These variables include: treatment status, gender, race/ethnicity, baseline measures of family size, birth order, grade in previous school year, grade retention, absenteeism, expectations of post-high school education, problem with English, enjoyment of reading and math, reading and math scaled MAT scores and demonstration site dummy variables.

Table V.1

SELECTED CHARACTERISTICS OF THE RANDOMIZED AND LEARNING ANALYSIS SAMPLES FOR COHORT III

	Randomized Sample			Learning Sample		
Characteristic	Treatment	Control	Total	Treatment	Control	Total
Gender						
Male	45%	52%	49%	44%	51%	47%
Female	55	48	51	56	49	53
Age						
≤ 14 years old	58%	56%	57%	59%	57%	58%
≥ 15 years old	42	44	43	41	43	42
Race/Ethnicity						
Asian	13%	16%	17%	20%	17%	18%
Black	47	52	50	47	53	50
Hispanic	20	18	19	19	17	18
White	11	11	11	11	11	11
Other	4	3	3	3	2	3
Sample Size	797	794	1591	638	613	1,251

Table V.2

DEMOGRAPHIC AND ECONOMIC CHARACTERISTICS OF COHORT III

	All Sites	Boston	Fresno	Portland	San Diego	Seattle
Demographic Characteristics						
Age						
≤ 14 Years Old	58%	65%	48%	61%	64%	50%
≥ 15 Years Old	42	35	52	39	36	50
Gender						
Male	47%	42%	51%	51%	44%	49%
Female	53**	58	49	49	56	51
Race/Ethnicity[a]						
Asian	18%	7%	14%	11%	32%	28%
Black	50	61	30	53	45	62
Hispanic	18	18	51	3	18	2
White	11	8	5	31	3	5
Other	3	6	0	2	2	3
Percentage Speaking a Language in Addition to English	42%	35%	52%	25%	59%	39%
Percentage Who Speak a Second Language with a Language Problem	32%	28%	20%	26%	44%	40%

Table V.2 - (cont'd)

DEMOGRAPHIC AND ECONOMIC CHARACTERISTICS OF COHORT III

	All Sites	Boston	Fresno	Portland	San Diego	Seattle
Economic Characteristics						
Percentage from Female-Headed Households[b]	49%	56%	46%	54%	45%	44%
Mean Household Size[c]	5.05	5.05	5.13	4.66	5.49	4.93
Number with Children	9	3	0	2	3	1
Number Married	6	2	0	2	0	2
Percentage Never Worked[d]	66%	61%	68%	63%	76%	66%
Sample size	1,251	243	253	271	256	228

[a] Across sites, the racial composition was significantly different at the 0.01 level.

[b] Across sites, the percentage of youth from female-headed households differed significantly at the 0.05 level.

[c] Across sites, household size differed at the 0.01 level.

[d] Across sites, the percentage of youth with work experience was significantly different at the 0.05 level.

**Treatment/control difference is significant at the 0.05 level.

Gender

Fifty-three percent of the Cohort III members were female and females were disproportionately represented among treatment youth. Within the treatment group, 56 percent were female compared with only 49 percent of the control group. This difference between treatments and controls is statistically significant at the 0.05 level.

Racial/Ethnic Composition

As in previous demonstration years, the largest racial/ethnic group represented in Cohort III was blacks, who make up 50 percent of the sample. Asians and Hispanics were equally represented, with both groups accounting for 18 percent of the overall sample. Whites constituted 11 percent of Cohort III, an increase of two percentage points over Cohort II. Three percent of the youth came from some other racial/ethnic background.

While blacks were the largest racial/ethnic group represented in four sites, Hispanics were the largest group served in Fresno. Fifty-one percent of Fresno's Cohort III enrollment were Hispanic, a percentage that is reflective of the local population.

Racial/ethnic compositions within sites remained fairly consistent between Cohort II and Cohort III in all sites except Fresno. Fresno's Hispanic enrollment increased seven percentage points (from 44% to 51%) and its enrollment of whites and youth from "other" backgrounds dropped from 12 percent to 5 percent. As with Cohorts I and II, the largest concentrations of Asians were found in the San Diego and Seattle programs with 32 and 28 percent, respectively.

Language Barriers

The ethnic diversity of the STEP sample produces a population that is highly multilingual. Forty-two percent of the youth in STEP's Cohort III report speaking a language in addition to English. Consistent with their high enrollments of Hispanics and Asians, Fresno and San Diego enrolled the highest proportion of youth who speak a second language. Nearly one-third of the youth who speak a second language report having a problem either in reading and writing English or in understanding teachers who instruct in English.

Household/Economic Status

All STEP youth are considered economically disadvantaged by virtue of their eligibility for federally funded JTPA programs. About half of these youth, 49 percent, are further disadvantaged because they come from one-parent, female-headed families--a background that typically signals economic burden. Site differences in the percentage of youth from female-headed families were significant at the 0.05 level; there was a much higher percentage of such youth in Boston and Portland--56 and 54 percent, respectively--than in other sites, where percentages were between 44 and 46 percent.

The average household size for Cohort III youth was five persons. Nine youth were already parents when they entered the program and six youth were already married.

For two-thirds of Cohort III youth, STEP provided the first paid work experience, an indication of the limited employment opportunities available to youth of this age group, especially to those from disadvantaged socioeconomic backgrounds. In San Diego, only 24 percent of the youth had prior work experience, significantly below rates seen in Boston, Portland and Seattle, which were in the 30 to 40 percent range.

Educational Ability

STEP youth have serious deficiencies in basic skills. On average, scores on the MAT administered at baseline indicated that Cohort III youth read at the sixth-grade level and have seventh-grade level mathematical skills. Since 41 percent of the youth were enrolled in the ninth grade or above, as shown in Table V.3, test scores indicate that many youth are performing below their grade of enrollment by as many as three grade levels in reading and two grade levels in math. This is true for both treatments and controls, since average pretest scores were not significantly different between the two groups.

Baseline scores in both reading and math varied significantly among sites. Youth in Seattle had the lowest average scores in reading (4.8 grade level), followed by youth in San Diego (5.3 grade level). It is interesting that the two sites with lowest reading scores have the highest enrollment of Asian youth; this suggests that reading scores may reflect an English-as-a-second-language phenomenon. Scores in the other three sites ranged from 6.2 in Fresno to 7.1 in Portland.

In math, youth in Seattle again had the lowest scores, with an average score at the 5.9 grade level. Cohort III youth in Fresno had the highest math ability levels, with scores averaging just under the eighth-grade level. As was the case for reading scores, math scores were significantly different across sites at the 0.01 level.

Table V.3

EDUCATIONAL ABILITY AND SCHOOL PARTICIPATION CHARACTERISTICS OF COHORT III

	All Sites	Boston	Fresno	Portland	San Diego	Seattle
Educational Ability						
Mean MAT Reading Score	721	727	724	743	708	699
(Grade Equivalent)[a]	(6.0)	(6.3)	(6.2)	(7.1)	(5.3)	(4.8)
Mean MAT Math Score	712	705	736	716	727	670
(Grade Equivalent)[a]	(7.0)	(6.8)	(7.9)**	(7.2)	(7.5)	(5.9)
Grade in School						
8th Grade & Below	59%	90%	46%	53%	58%	48%
9th Grade	36	10	44	45	38	42
10th Grade & Above	5	0	10	2	4	10
Grade Retention History						
Percentage Ever Repeating a Grade[b]	33%	46%	29%	26%	30%	37%
Absenteeism[c]						
Percentage Never Absent	14%	15%	9%	8%	22%	14%
Percentage Absent 1-10 Days in SY 86-87	60	64	60	63	57	56
Percentage Absent 11-20 Days in SY 86-87	17	14	22	17	13	20
Percentage Absent More Than 20 Days in SY 86-87	9	7	8	12	8	10

Table V.3 - (cont'd)

EDUCATIONAL ABILITY AND SCHOOL PARTICIPATION CHARACTERISTICS OF COHORT III

	All Sites	Boston	Fresno	Portland	San Diego	Seattle
Educational Expectations						
Percentage Planning to Pursue Post-High School Education[d]	82%	76%	86%	84%	83%	83%
Sample size	1,251	243	253	271	256	228

[a] Across sites, reading and math scores differed at the 0.01 level.

[b] Across sites, grade retention rates differed at the 0.01 level.

[c] Across sites, absenteeism rates differed at the 0.05 level.

[d] Across sites, educational expectation levels differed at the 0.05 level, influenced by the substantially lower levels displayed by Boston's youth.

**The treatment/control difference is significant at the 0.05 level.

Grade of Enrollment

Depending on their date of birth, STEP youth who are making normal progress should be enrolled in the eighth, ninth or 10th grades. As Table V.3 shows, 59 percent of Cohort III youth were enrolled in the eighth grade. The mean grade of enrollment across all sites is 8.37. As in previous years of the demonstration, Boston's emphasis on serving exiting eighth-graders influences the high representation of eighth-graders in the sample. Ninety percent of the youth in Boston were enrolled in the eighth grade at the end of the 1987 school year. This differs significantly from the other sites, where there is greater representation of youth from the ninth and tenth grades.

Grade Retention History

One-third of Cohort III youth have been retained in at least one grade. The highest grade retention rate was 46 percent in Boston; the lowest rate was 26 percent in Portland.

Absenteeism

Attendance data for STEP analyses are collected from two sources: self-reported information and school data. We found last year that self-reported information on Cohort II attendance was severely underreported--by approximately 40 percent. For Cohort III, however, self-reported information is currently our only source of attendance data.[36]

More than one-quarter of Cohort III youth reported absences in excess of ten days during the 1986-87 school year. (Based on our experience with school data for the first two cohorts, we believe that a better estimate of youth with absences of more than ten days is approximately 48 percent.) Sixty percent of youth reported that they were absent from school between one and 10 days. Fourteen percent reported that they were never absent during the school year.

Educational Expectations

As a group, Cohort III youth claimed the highest expectations for pursuing post-high school education of STEP cohorts to date. Eighty-three percent of Cohort III youth expect to pursue some form of education after completing high school, compared with rates of 71 and 72 percent for Cohorts I and II, respectively. Expectations for post-high school education were fairly consistent across the Fresno, Portland, San Diego and Seattle sites. However, youth in Boston had expectations that were significantly lower, with only 76 percent expecting to continue their education beyond high school.

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[36] School data on Cohort III will be collected for the 1987-88 school year and will be available for next year's report. At that time, we can reexamine the relationship between self-reported data and school data for these youth.

Fertility-Related Characteristics

As discussed in Chapter IV, the classroom component of STEP includes both academic instruction and instruction in life skills. This section describes the Cohort III sample in terms of their attitudinal, behavioral and knowledge characteristics on issues central to the life skills component; issues that primarily concern sexuality.

To determine knowledge, attitudes and behaviors of youth with respect to fertility-related issues, a series of questions are asked at baseline.[37] Response rates and sample sizes on these items differ from those noted previously in this chapter; sample sizes for these variables are noted on Table V.4.

Forty-four percent of Cohort III youth reported being sexually experienced before joining the STEP program. The proportion of youth who were sexually experienced varied significantly across sites, ranging from a low of 31 percent in San Diego to a high of 52 percent in Portland. Also significant was the difference between controls and treatments: 48 percent of controls had sexual experience, compared with 41 percent of the treatments. On average, youth who were sexually experienced began sexual activity at the age of 12.

Of youth reporting sexual experience, 44 percent were sexually active in the two months prior to program start-up. Thirty-seven percent of recently sexually active youth said they never used any form of contraception during that two-month period. Twenty-seven females and 12 males in Cohort III reported having been pregnant or being responsible for a pregnancy prior to enrolling in STEP.

Along with limited use of contraception, Cohort III youth displayed limited knowledge on issues related to birth control. On a series of eight items testing their knowledge of the availability of various contraceptive methods, youth correctly answered an average of only three. On 12 general birth control knowledge

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[37] In consideration of the sensitive nature of many of the questions, questionnaires are processed in a way that ensures the confidentiality of responses. Identification numbers are used on questionnaires rather than names and completed forms are sealed in envelopes by respondents. These measures are taken to ensure confidentiality, but they also serve to improve the rates of response on typically high non-response items.

Table V.4

FERTILITY-RELATED CHARACTERISTICS OF COHORT III

Characteristic	All Sites	Boston	Fresno	Portland	San Diego	Seattle
Percentage With Prior Sexual Experience[a]	44%***	48%	41%	52%*	31%*	51%
Percentage of the Sexually Experienced Sample Who Were Sexually Active in the Past Two Months	44%	52%	36%	43%	42%	47%
Percentage of Recently Active Sample Never Using Contraception in the Past Two Months	37%	46%	44%	29%	46%	24%
Mean Age at Onset of Sexual Activity for the Sexually Experienced Sample[b]	12.2	12.1	12.6	12.4	12.0	11.9
Number of Females Ever Pregnant	27	6	4	4	3	10
Number of Males Ever Fathered a Child or Responsible for a Pregnancy	12	4	1	1	4	2
Mean Number Correct on Birth Control Availability Questions (8-item scale)[c]	3.0	3.0	3.2	3.0	2.9	3.0

Table V.4 - (cont'd)

FERTILITY-RELATED CHARACTERISTICS OF COHORT III

Characteristic	All Sites	Boston	Fresno	Portland	San Diego	Seattle
Mean Number Correct on General Birth Control Knowledge Questions (12-item scale)[c]	5.4	5.3	6.0*	5.6	4.8	5.2
Percentage Believing That Both Partners Should Be Responsible for Birth Control	78%	74%	78%	84%	74%	78%
Percentage Who Took a Prior Sex Education Course	73%	62%	74%	72%	85%	70%

Note: Across all sites, 1,201 youth answered the sexual experience question; 601 answered the question on sexual activity within the last two months; 262 answered the contraception question; 536 answered the question on age of sexual initiation; 258 females and 379 males answered the question on pregnancy; 1,234 answered the questions on contraception availability; 1,231 answered the questions on general birth control knowledge; 1,234 answered the question on responsibility for birth control; and 1,204 answered the question on taking a prior sex education course.

[a] Across sites, differences in sexual experience levels were significantly different at the 0.01 level.

[b] Across sites, differences in age of onset of sexual activity differed at the 0.10 level.

[c] Differences in knowledge of general birth control issues and of birth control availability issues were significant at the 0.01 level across sites.

***The treatment/control difference is significant at the 0.01 level.
*The treatment/control difference is significant at the 0.10 level.

items, an average of five were correctly answered. Although there were significant differences on both measures across sites, no overall differences between treatments' and controls' scores were found. In Fresno, however, controls answered significantly more (6.28 items) general knowledge items correctly than did treatments (5.72 items).

Despite these indications of limited birth control knowledge, 73 percent of the youth reported that they had a sex education course prior to STEP. At baseline, 75 percent of control group youth and 71 percent of treatment youth reported having taken a sex education course.

Summary of Cohort III Baseline Characteristics

Baseline data indicate that Cohort III youth are a group whose need for an intervention like STEP is evident. With below-grade-level test scores (youth average a sixth-grade level in reading and seventh-grade level in math), frequent grade retention histories and sporadic school attendance patterns, these youth are likely to drop out of school unless a program designed to improve their academic skills and foster better school participation behavior is made available. Adding to these academic deficiencies is a limited knowledge of birth control and an early and high incidence of sexual activity, factors that underscore the need for STEP's LSO training.

COHORT II TREATMENT RETURNEES

In this section, we look at the characteristics of the youth who returned for a second summer of STEP in 1987. In all sites, youth who joined Cohort II of STEP and were randomized to the treatment group were eligible to return for a second summer of STEP services. In San Diego and Seattle only, members of the Cohort II control group were also invited back for a second summer of work experience. We examined the characteristics of youth who took advantage of their second-summer opportunities in order to determine whether they differ from youth who chose not to return. As stated earlier, data used to describe returnees were collected at baseline for these youth--in June and July 1986.

In 1986, 819 youth were randomized to the treatment group of Cohort II. Ninety-three percent of these youth (765) were pretested and 91 percent of the pretested sample (698) were posttested following their first summer. In 1987, 611 treatment youth indicated an intention to return for a second summer of STEP. Of these, 558 (or 91%) showed up for the pretest and constitute the sample for the descriptions that follow.

Demographic and Economic Characteristics

Comparisons of Cohort II returnees and non-returnees on demographic, economic, educational and fertility-related measures are presented in the following sections. Demographic and economic comparisons were conducted on age, gender, racial/ethnic composition, language barriers and economic factors.

Age

Before implementing STEP, we had hypothesized that younger youth would be more likely to return for a second summer of service because of the relatively few employment opportunities available to them. However, as shown in Table V.5, a significantly greater proportion of Cohort II youth who joined the program at age 15 returned for a second summer than did those who joined as 14-year-olds. The reasons for this pattern, which represents a drastic change from Cohort I, are unknown.

Gender

Consistent with the 1986 experience, more females returned for a second summer of STEP in 1987 than males. This was true overall and within each site. Overall, 55 percent of the returning cohort were female. In Fresno, the proportion of returnees who were male (48%) was significantly different from the proportion of non-returnees who were male (64%).

Racial/Ethnic Composition

Race and ethnicity continued to influence the decision to return for a second summer of service. As with Cohort I, Asians were more likely to take advantage of the second- summer opportunity than not; Asians constituted 20 percent of returnees, but only 14 percent of non-returnees. The proportion of returnees who were black was also significantly above the corresponding figure for non-returnees. Blacks constituted 48 percent of returnees and 42 percent of the non-returnees.

San Diego was the only site in which the proportion of Hispanic returnees was significantly different from the proportion of Hispanic non-returnees. Hispanics in San Diego were less likely to return for a second summer of STEP--only 15 percent of returnees in San Diego were Hispanic, compared with 30 percent of non-returnees.

Youth from "white and other" backgrounds were also less likely to return to STEP. Twenty-three percent of the youth who decided not to take advantage of a second summer of STEP were from this classification, compared with only 14 percent of returnees. This difference is statistically significant at the 0.01 level.

Table V.5

DEMOGRAPHIC AND ECONOMIC CHARACTERISTICS OF THE COHORT II RETURNING TREATMENTS

	All Sites		Returnees				
	Non-Returnees	Returnees	Boston	Fresno	Portland	San Diego	Seattle
Demographic Characteristics							
Age							
≤ 14 Years Old	52%	44%	37%	42%	37%	51%	50%
≥ 15 Years Old	48**	56	63	58	63**	49	50
Gender							
Male	49%	45%	42%	48%*	48%	46%	43%
Female	51	55	58	52	52	54	57
Race/Ethnicity							
Asian	14%	20%	9%	11%	13%	38%***	27%**
Black	42	48	59 *	34 *	53 ***	42	54
Hispanic	22	18	21	49	4	15 **	2
Whites & Others	23	14	10	6 **	30 ***	5	17
Percentage with Limited English Proficiency	30%**	34%	29%	36%	18%	30%**	33%

Table V.5 - (cont'd)

DEMOGRAPHIC AND ECONOMIC CHARACTERISTICS OF THE COHORT II RETURNING TREATMENTS

	All Sites		Returnees				
	Non-Returnees	Returnees	Boston	Fresno	Portland	San Diego	Seattle
Economic Characteristics							
Percentage from Female-Headed Households	52%	46%	59%	34%	44%	43%	52%
Mean Household Size	4.88**	5.23	4.99	5.28	4.61	6.25**	5.05***
Number with Children	4	6	1	0	0	0	5
Percentage Never Worked	59%***	71%	68%	71%	61%**	87%**	70%

***The difference between returnees and non-returnees on this variable is significant at the 0.01 level.
**The difference between returnees and non-returnees on this variable is significant at the 0.05 level.
*The difference between returnees and non-returnees on this variable is significant at the 0.10 level.

Language Barriers

Except in San Diego, language problems or limited English proficiency did not show an influence on the decision to return to STEP for a second summer. In San Diego, the proportion of returnees who reported language problems was significantly above that of the non-returnees. Thirty percent of returnees and 13 percent of non-returnees in San Diego had problems with English.

Household/Economic Status

Youth from female-headed households were more heavily represented among the non-returnees. Forty-six percent of those returning to STEP came from families headed by a female, compared with 52 percent of the non-returnees. However, the difference between returnees and non-returnees on this variable is not statistically significant.

Another descriptor on which the two groups differed was average household size. Returnees were significantly more likely to come from larger families. The average household size for returnees is 5.22 persons, compared with 4.88 persons for non-returnees.

The final economic indicator that we examined was previous work experience. As would be expected based on last year's findings, returnees tended to be those youth who did not have prior work experience. Less than one-third (29%) of those returning held paying jobs prior to their affiliation with STEP, compared with 41 percent of those who did not return.

Educational Ability

Overall, returnees had higher baseline scores in both reading and math than non-returnees. As Table V.6 shows, returnees scored five-tenths of a grade level above those who did not return; in math, returnees scored four-tenths of a grade level higher. Scores between the two groups are significantly different only in math, however.

School Participation and Educational Expectations

Grade of enrollment did not differ between returnees and non-returnees. However, non-returnees were more likely to have been retained than returnees. Thirty-one percent of the returnees had a history of grade retention, compared with 38 percent of the non-returnees.

Self-reported school attendance patterns also varied significantly between the two groups. Returnees tended to report absenteeism in the lower range of 0 to 10 days at a higher rate; they were also less likely to report having been absent 11 or

Table V.6

EDUCATIONAL ABILITY AND SCHOOL PARTICIPATION CHARACTERISTICS OF COHORT II RETURNING TREATMENTS

	All Sites		Returnees				
	Non-Returnees	Returnees	Boston	Fresno	Portland	San Diego	Seattle
Educational Ability							
Mean MAT Reading Score[a]	713	721	703	724	719	732	724
(Grade Equivalent)	(5.6)	(6.0)	(5.1)	(6.2)	(5.9)	(6.6)	(6.2)
Mean MAT Math Score[b]	685	700	669	699	702*	737*	695
(Grade Equivalent)	(6.3)***	(6.7)	(5.9)	(6.6)	(6.7)	(7.9)	(6.5)
Grade in School							
8th Grade	61%	59%	94%	49%	54%	51%	50%
9th Grade	33	36	5	46	44	44	36
10th Grade & Above	6	5	1	5	2	5	14
Grade Retention History							
Percentage of Full Sample Ever Repeating a Grade	38%**	31%	44%	36%**	28%	23%	25%***
Absenteeism							
Percentage Never Absent	9%*	14%	21%	10%	10%	21%	11%
Percentage Absent 1-10 Days in SY 85-86	57 *	64	56	63 **	67	63	68
Percentage Absent 11-20 Days in SY 85-86	21 **	14	17	14 *	18	11	11
Percentage Absent More Than 20 Days in SY 85-86	13 **	8	6	13	5	5	10

Table V.6 - (cont'd)

EUDCATIONAL ABILITY AND SCHOOL PARTICIPATION CHARACTERISTICS OF COHORT II RETURNING TREATMENTS

	All Sites		Returnees				
	Non-Returnees	Returnees	Boston	Fresno	Portland	San Diego	Seattle
Educational Expectations							
Percentage Planning to Pursue Post-High School Education[c]	66%	71%	57%	74%	74%	74%	74%
Sample Size	261	558	96	121	112	107	122

[a] Across sites, mean test scores in reading differed at the 0.05 level.

[b] Across sites, mean test scores in math differed at the 0.01 level.

[c] Across sites, expectation levels differed at the 0.05 level.

***The difference between returnees and non-returnees on this variable is significant at the 0.01 level.
**The difference between returnees and non-returnees on this variable is significant at the 0.05 level.
*The difference between returnees and non-returnees on this variable is significant at the 0.10 level.

more days during the school year, according to self-reported figures.

Fertility-Related Characteristics

Consistent with findings from Cohort I, Table V.7 shows that a majority of Cohort II returnees were youth who were not sexually experienced at baseline. While 52 percent of the non-returnees reported being sexually experienced, only 40 percent of those returning to the program made a similar report. This difference between the two groups is significant at the 0.01 level. There was also wide variation across sites in the percentage of returning youth with prior sexual experience; percentages ranged from 30 percent in San Diego to 51 percent in Portland.

While returnees and non-returnees differed with respect to their levels of sexual experience, the two groups were remarkably similar in terms of recent sexual activity, birth control usage, mean age of sexual initiation, birth control knowledge and exposure to sex education. Finally, females who reported ever being pregnant were exclusively returnees.

COHORT II CONTROL RETURNEES

As noted earlier, the San Diego and Seattle sites offered a second summer of work experience to control youth from the second cohort of STEP. A total of 218 control youth returned, 113 to the Seattle program and 105 returned to the San Diego program. This return rate of 90 percent is comparable to the percentage of treatments who returned. Tables presenting the characteristics of the returning control groups in Seattle and San Diego appear in Appendix Tables B.1 to B.6.

Control youth who took advantage of a second summer in San Diego and Seattle were virtually identical to those who did not return in terms of demographic, economic, educational and fertility-related characteristics. Only in San Diego were differences found; returning controls in San Diego had scores that were significantly higher than non-returnees in both reading and math. Also, only 22 percent of returnees in San Diego had any work experience prior to their affiliation with STEP, compared with 37 percent of non-returnees. Finally, 60 percent of the non-returnees in San Diego were sexually experienced, compared with only 38 percent of the returnees. This difference is significant at the 0.01 level.

THE WAVE I FOLLOW-UP INTERVIEW SAMPLE

As discussed in Chapter II, the research design for STEP includes follow-up surveys of both treatment and control youth in four data collection waves. The first wave commenced in October 1987,

Table V.7

FERTILITY-RELATED CHARACTERISTICS OF COHORT II RETURNING TREATMENTS

	All Sites		Returnees				
	Non-Returnees	Returnees	Boston	Fresno	Portland	San Diego	Seattle
Percentage With Prior Sexual Experience[a]	52%***	40%	40%	35%***	51%	30%*	43%
Percentage of the Sexually Active Sample Who Were Sexually Active in the Past Two Months	51%	52%	47%	54%	48%	63%	50%
Percentage of Recently Active Sample Never Using Contraception in the Past Two Months	20%	22%	26%	39%	13%	28%	9%
Mean Age at Onset of Sexual Activity for the Sexually Active Sample	12.0	12.1	11.7	12.4	11.9	12.2	12.3
Number of Females Ever Pregnant	0	10	1	0	2	0	7
Number of Males Ever Fathered a Child or Responsible for a Pregnancy	5	4	0	0	0	0	4
Mean Number Correct on Birth Control Availability Questions (8-item scale)	3.08	2.99	2.66	3.04	2.98	3.06	3.16

Table V.7 - (cont'd)

FERTILITY-RELATED CHARACTERISTICS OF COHORT II RETURNING TREATMENTS

	All Sites		Returnees				
	Non-Returnees	Returnees	Boston	Fresno	Portland	San Diego	Seattle
Mean Number Correct on General Birth Control Knowledge Questions (12-item scale)	4.98	5.02	4.42	5.34	5.14	4.85	5.21
Percentage Believing That Both Partners Should Be Responsible for Birth Control	70%	74%	63%	72%	74%	76%**	82%
Percentage Who Have Taken a Prior Sex Education Course[b]	72%	69%	58%	60%	79%	88%	62%**

[a] Across sites, the percentage of returnees with sexual experience differed at the 0.05 level.

[b] Across sites, the percentage of returnees who have taken a sex education course differed at the 0.05 level.

***The difference between returnees and non-returnees on this variable is significant at the 0.01 level.
**The difference between returnees and non-returnees on this variable is significant at the 0.05 level.
*The difference between returnees and non-returnees on this variable is significant at the 0.10 level.

and includes surveys of youth from the first two cohorts. Subsequent waves will focus on youth from Cohorts II and III.

In this section, the characteristics of youth who participated in the first wave of interviews are examined to determine whether there are significant differences between the youth who were interviewed and the youth who completed tests and questionnaires at baseline. Again, all data used in this section were collected at baseline, i.e., June 1985 for Cohort I youth and June 1986 for Cohort II youth.

Cohort I Interview Sample

Eighty-four percent of the Cohort I randomized sample (1,336 youth) were interviewed during the first wave of longitudinal surveys.[38] Of these, 1,113 were included in the learning analysis sample and are the focus of the descriptions provided in this section.

As might be expected from the high rate of response, the first wave interview sample is very representative of the youth in the original sample. Good representation was also achieved in terms of treatment/control group assignment and site affiliations. Half of those interviewed were treatment group youth and half were controls. Boston, Fresno and Portland each provided 20 percent of the interview sample, while 19 percent were from San Diego and 21 percent were from Seattle.

As Table V.8 shows, the characteristics of Cohort I survey respondents mirror those of the original sample in terms of basic demographic, educational and fertility-related measures. Half of the youth were 14 years old at the time of entry; 52 percent were male; approximately half came from female-headed households; and the average household size for participation was of 5.29 persons. Racial/ethnic distributions of those interviewed were nearly identical to that of the original sample, as were baseline test scores. One difference was found between interviewed and non-interviewed youth; non-interviewed youth were significantly more likely to be sexually experienced at baseline.

Cohort II Interview Sample

Interviews were conducted with 1,405 Cohort II youth, 86 percent of those randomized to STEP's second cohort. Of these, 1,275 youth constitute the learning analysis sample and are the basis for this section's discussion. As with the first cohort, sites

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[38] These numbers refer to surveys completed as of January 31, 1988. However, interviews are continuing with members of the Wave I sample through June.

Table V.3

COMPARISON OF THE WAVE I INTERVIEW SAMPLE TO THE ORIGINAL SAMPLE ON SELECTED CHARACTERISTICS

	Cohort I		Cohort II	
	Interview Sample	Original Sample	Interview Sample	Original Sample
Demographic Characteristics				
Age				
≤ 14 years old	50%	50%	45%	55%
≥ 15 years old	50	50	55	45
Gender				
Male	52%	53%	53%**	49%
Female	48	47	47	51
Race				
Asian	15%	14%	18%	19%
Black	58	59	47	48
Hispanic	16	16	18	18
White & Other	11	11	16	15
Household Size	5.29	5.23	5.12	5.16
Percentage from Female-Headed Households	49%	50%	49%	52%
Educational Characteristics				
Mean MAT Reading Score	711	717	721	722
(Grade Equivalent)	(5.5)	(5.7)	(6.0)	(6.0)
Mean MAT Math Score	687	690	702	703
(Grade Equivalent)	(6.3)	(6.4)	(6.7)	(6.7)
Mean Grade of Enrollment	8.5	8.5	8.4	8.3
Percentage Repeating a Grade	28%	28%	32%	31%
Percentage Absent More Than 10 Days in the School Year Preceding STEP	27%	27%	24%	27%
Fertility-Related Characteristics				
Percentage Sexually Experienced	48%***	51%	43%	42%
Mean Age of Sexual Initiation	11.7	11.7	12.1	12.1
Percentage Sexually Active in the Two Months Preceding Baseline	27%	29%	50%	53%

***The difference between interviewed and non-interviewed youth on this variable is significant at the 0.01 level.

**The difference between interviewed and non-interviewed youth on this variable is significant at the 0.05 level.

were fairly evenly represented in follow-up interviews. Youth in Fresno were the most frequently interviewed--22 percent. Boston and San Diego each provided 20 percent of interviewees, while Portland and Seattle sites each provided 19 percent. Fifty-one percent of those interviewed were treatment youth and 49 percent were controls.

Youth in the Cohort II interview sample were also very similar to the original group in terms of their demographic, educational and fertility-related characteristics. Age and gender distribution were exceptions. Only 45 percent of the original sample was 15 years old at program entry compared with 55 percent of those interviewed. Finally, the proportion of males interviewed was significantly lower than the proportion of males among the non-interviewed sample (47% versus 60%).

SUMMARY OF PARTICIPANT CHARACTERISTICS

The Cohort III Sample

Cohort III youth fit the profile of dropout-prone youngsters. They are educationally and economically disadvantaged, have had negative school experiences (grade retention, poor basic skills) that often lead to dropout behavior, and many are already sexually active.

These youth have very similar baseline characteristics to those of Cohort II youth at baseline. Like Cohort II youth, a majority are female, they are racially and ethnically diverse, and they possess substandard basic skills. Their school participation characteristics were similar to Cohort II at baseline and comparable proportions of the two cohorts were sexually experienced since the age of 12. The two cohorts' contraceptive knowledge and behavior were also similar.

However, the two cohorts of youth were dissimilar in two respects. More youth from Cohort III (83%) expected to pursue post-high school education than from Cohort II. Also, almost twice as many females from Cohort III reported having been pregnant (27 versus 14).

The Cohort II Returning Treatments

Cohort II youth who returned for a second summer of STEP services differed significantly from those who did not in several respects. In general, non-returnees were more "at risk" than returning youth. Non-returnees had lower average test scores, were more likely to have been retained in one or more grades, had higher rates of reporting absences of more than 10 days, and reported lower educational expectations. Non-returnees were also significantly more likely to be sexually experienced.

Returning youth were older than non-returnees, were more likely to be Asian or black, and were less likely to be white. They had higher test scores in both reading and math, came from larger families, were more likely to be inexperienced in the workplace, and were less likely to have had a course in sex education.

Cohort II Returning Controls

Only two sites offered a second summer of work experience to control youth in 1987--San Diego and Seattle. Very few differences were evident between control returnees and non-returnees in these sites. Only in San Diego did differences emerge--basic skill levels were higher among returnees, while work experience history and level of sexual experience were lower.

Also, few patterns emerged with respect to who the control returnee is likely to be because returnees in the two sites differed in fundamental respects. For example, while older youth returned more frequently in the San Diego program, younger youth did so in Seattle. More males returned in Seattle, but more females returned in San Diego. Finally, returning youth in Seattle had scores in reading and math that were almost equivalent to non-returnees, while scores for returning youth were higher than for non-returnees in San Diego.

VI. FIRST-SUMMER IMPACTS--COHORT III AND AGGREGATE SAMPLES

The long-term objectives of STEP are increasing participants' high school graduation rates and reducing their teen parenting rates. STEP strives to achieve these long-term goals by providing remediation and life skills classes, work experience and school-year support. The program's short-term success is measured by determining changes in participants' academic skill levels, school retention rates, knowledge of the consequences of adolescent pregnancy, and knowledge and use of contraception after one summer of participation. Changes are compared with those seen for the control group.

In this chapter, we present analyses of the first-summer impacts of STEP with respect to academic and life skills outcomes. As discussed previously, STEP participants entered the demonstration in three waves--the first cohort in 1985, the second cohort in 1986 and the third cohort in 1987. First-summer impacts for Cohorts I and II were published earlier (Branch, et al., 1986 and Sipe, et al., 1987). The primary focus of the analyses presented here is the short-term impacts of STEP participation on Cohort III youth. First-summer impacts are also analyzed for an aggregated sample of Cohorts II and III.[39] Results for this aggregated sample are presented in Appendix tables B.7, B.8 and B.12 through B.15, with discussion here limited to differences from the Cohort III results.

In general, results were positive. Cohort III treatment youth performed significantly better than Cohort III controls, experiencing significant gains in both math and reading during their first summer in STEP. However, participation in one summer of STEP did not affect most other education-related outcomes, including educational expectations, enjoyment of reading and math, and school enrollment rates for the 1987-88 school year. With respect to fertility-related outcomes, treatment youth improved their knowledge of both contraception and the consequences of adolescent pregnancy over the summer significantly more than controls did.

While Cohort III results were positive, they were not quite as impressive as those observed for Cohort II youth after their first summer of participation. However, outcomes for Cohort III were much stronger than those observed for Cohort I.

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[39] As discussed in Chapter II, the analyses focus on Cohorts II and III because we feel that their experience in the program represents a "best test" of the STEP model.

ACADEMIC PERFORMANCE

In the following sections, the impact of STEP participation on a number of measures of Cohort III youth's academic performance are discussed. Impacts presented are limited to those that could be detected after one summer of program participation.[40]

The primary outcome measures of academic performance are the scaled test scores in reading and math produced by the Metropolitan Achievement Test (MAT), which both treatment and control youth are given at baseline and at the end of the summer. Scaled test scores are the result of the conversion of raw scores (the number of correct answers on the test) to a metric that is more suited to statistical analysis. Although scaled scores are used for analysis, the results are often converted to grade equivalent scores after the analysis has been completed, thus providing an interpretive context for the scaled scores.[41]

We analyzed the scaled test scores using two methods. The first method involves a comparison of changes from pretest to posttest for treatment and control youth using simple unadjusted means. These unadjusted means depict actual test scoring and do not include statistical adjustments for pretreatment differences between treatment and control group youth on important variables. While they provide descriptive measures that give the reader a feel for the data, they were not used to determine the program impacts. Instead, a second analysis method, known as multiple regression, was used. Regression analysis provides a more precise estimate of the impact by making necessary statistical adjustments.[42] If, for example, results of the regression analysis show a net impact of 10 points in reading, this would indicate that treatment youth would outscore control youth by an average of 10 points at posttest if baseline characteristics are been identical.

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[40] Impacts observed following the full 15 months of STEP participation for Cohorts I and II are presented in Chapters VII and VIII.

[41] For the MAT reading test, a change of 10 scale points is equivalent to roughly half a grade level; for the math test, a change of 20 points represents approximately half a grade level. However, "grade equivalent units are not equal throughout the scale. An increase in raw score of 10 points may amount to only three months of change in one part of the scale and three years in another part of this scale" (MAT Teacher's Manual, 1978:27).

[42] The regression coefficient on the treatment variable is an estimate of the impact of STEP controlling for baseline differences between treatment and control youth. The explanatory variables included in the regression equations, which are discussed in Chapter III, are identical to those in the models estimated for Cohort II.

Before analyses were conducted, the nature of sample attrition was explored. Results of a logit analysis indicated that attrition from the randomization sample to the analysis sample was greater among control youth than treatment youth. Treatments were seven percent more likely to be part of the learning analysis sample than controls. This differential attrition introduces sample-selection bias that is statistically corrected for in the multivariate models.[43]

Mean Test Score Changes

Changes from pretest to posttest in mean test scores are presented for both treatment and control youth in Cohort III in Tables VI.1 and VI.2. Mean reading and math scores at pretest and posttest were calculated separately for treatment and control groups, and were broken down by gender, race/ethnicity and site. We tested whether pretest to posttest changes within each group were statistically different from zero and whether the degree of change experienced by treatment youth was significantly different from that experienced by control youth. (Similar analyses were performed for the aggregate sample of Cohorts II and III, the results of which are presented in Appendix Tables B.7 and B.8. The aggregate results are generally a little stronger than the Cohort III treatment/control differences in test score changes.)

For the first time since the demonstration began, the results of the learning analysis indicate that treatment youth experienced statistically significant gains of 4.06 points in reading over the summer (equivalent to 0.3 of a grade level). At the same time, the Cohort III control group experienced small but statistically significant losses. On average, Cohort III treatment youth outperformed control youth by 9.41 points in reading.

In math, treatment youth experienced a significant gain of 6.32 points (equivalent to 0.2 of a grade level). This gain, coupled with significant control losses, resulted in an average treatment/control differential of 15.72 points in math.

The subgroup means in Tables VI.1 and VI.2 indicate that the program was more effective for some groups than for others. Differential program impacts for gender, racial/ethnic, and site subgroups will be discussed in the context of the multivariate analysis.

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[43] For further discussion of the sample-selection bias problem and the correction factor, see Chapter III.

Table VI.1

MEANS OF PRETEST TO POSTTEST CHANGES IN READING AND MATH SCORES BY GENDER AND RACE/ETHNICITY FOR COHORT III

Group (n)	Reading Scores		Math Scores	
	Change from Pre- to Posttest	Treatment/Control Difference	Change from Pre- to Posttest	Treatment/Control Differences
Total Sample				
Treatment (638)	4.06**		6.32***	
Control (613)	-5.35***	9.41***	-9.40***	15.72***
Male				
Treatment (283)	-2.54		2.90	
Control (312)	-12.99***	10.45***	-14.32***	17.22***
Female				
Treatment (355)	9.33***		9.05***	
Control (301)	2.57	6.76***	-4.29	13.34***
Asian				
Treatment (126)	14.31***		6.66	
Control (102)	12.53***	1.78	10.47	-3.81
Black				
Treatment (302)	-0.01		4.45	
Control (323)	-10.18***	10.17***	-12.11***	16.56***
Hispanic				
Treatment (122)	9.32**		6.94	
Control (106)	3.31	6.01	-10.67	17.61*
White & Other				
Treatment (88)	-3.93		11.39	
Control (82)	-19.76***	15.83*	-21.80***	33.19***

***Difference is significantly different from zero at .01 level.
**Difference is significantly different from zero at .05 level.
*Difference is significantly different from zero at .10 level.

Table VI.2

MEANS OF PRETEST TO POSTTEST CHANGES IN READING AND MATH SCORES BY SITE FOR COHORT III

Site (n)	Reading Scores: Change from Pre- to Posttest	Reading Scores: Treatment/Control Difference	Math Scores: Change from Pre- to Posttest	Math Scores: Treatment/Control Differences
Total Sample				
Treatment (638)	4.06**		6.23***	
Control (613)	-5.35***	9.41***	-9.40***	15.72***
Boston				
Treatment (124)	-4.32		9.17*	
Control (119)	-19.68***	15.36**	-15.89***	25.06***
Fresno				
Treatment (129)	10.38***		2.98	
Control (124)	7.48*	2.90	-6.47	9.45
Portland				
Treatment (136)	-8.94*		11.10**	
Control (135)	-10.39**	1.45	-2.99	14.09*
San Diego				
Treatment (132)	21.06***		8.23	
Control (124)	5.16	15.90***	-8.63*	16.86**
Seattle				
Treatment (117)	1.93		-0.72	
Control (111)	-9.93*	11.86*	-14.37*	13.65

***Difference is significantly different from zero at 0.01 level.
**Difference is significantly different from zero at 0.05 level.
*Difference is significantly different from zero at 0.10 level.

Overall Impacts

The regression estimates presented in Table VI.3 confirm that STEP participation had a significant impact on both the reading and math scores of Cohort III youth.[44] The net impact of STEP on MAT reading scores for Cohort III was 9.33 points, or 0.5 of a grade equivalent. The net impact on math performance for Cohort III was 16.43 points, or 0.6 of a grade equivalent. Although Cohort III treatment youth in almost every subgroup performed better than their Cohort II counterparts in reading and about the same in math, net impacts are a bit smaller than those observed for Cohort II (0.6 of a grade equivalent in reading and 0.8 of a grade equivalent in math). These smaller impacts appear to be due almost entirely to smaller losses (and in some subgroups, gains) among the control group.

Participation in STEP had a significant impact on the test scores of Cohort III youth in most gender, racial/ethnic and site subgroups. The only exceptions to this general pattern were the absence of significant impacts in either reading or math among Asians and youth in Fresno, and the absence of significant impacts on reading scores among Hispanics and youth in Portland.[45] The effects of STEP seen for Cohort III were very similar to those seen for Cohort II.

Aggregate Sample Outcomes

The net impacts of STEP participation, which are presented in Table VI.3, were significant for the overall aggregate sample of Cohorts II and III, for both males and females, for virtually all racial/ethnic groups and for all five sites. Other than Asians and youth in Fresno, for whom participation in STEP significantly affected posttest math scores but not reading scores, all subgroups in the aggregate sample of Cohorts II and III improved their MAT scores by participating in STEP. In general, the magnitudes of these impacts are similar to those estimated for Cohort III.

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[44] The variables included in all learning-related multivariate analysis models are: treatment status, gender, race/ethnicity and grade, as well as baseline measures of family size, birth order, grade retention, absenteeism, expectations of post-high school education, problem with English, enjoyment of reading and math, pretest MAT scaled scores in reading and math and demonstration site dummy variables.

[45] Statistically, however, only the math impact for Asians varied significantly by race/ethnicity. There was no significant statistical difference in either reading or math impacts by site.

Table VI.3

NET IMPACT OF PARTICIPATION IN STEP ON MAT SCALED SCORES IN READING AND MATH FOR COHORT III AND THE AGGREGATE SAMPLE OF COHORTS II AND III

Group	Cohort III		Aggregate Sample	
	Reading	Math	Reading	Math
Overall	9.33***	16.43***	9.69***	18.76***
Gender				
Male	9.70***	17.00***	12.65***	22.16***
Female	8.80**	15.60***	6.94***	15.60***
Race/Ethnicity		##		
Asian	0.89	2.11	4.19	13.09***
Black	11.32***	17.11***	11.75***	20.17***
Hispanic	7.90	13.34*	7.69*	14.02***
White & Other	14.84**	36.61***	12.17***	27.04***
Site				
Boston	13.50**	23.73***	11.64***	20.23***
Fresno	3.72	6.16	3.62	12.75**
Portland	4.81	18.90***	7.29*	18.25***
San Diego	14.47***	19.14***	11.31***	15.09***
Seattle	10.71*	14.08*	14.99***	28.13***

Note: The sample size for the Cohort III analyses is 1,251 and 2,511 for the aggregate sample analyses. Impacts for each subgroup are computed as the sum of the treatment coefficient plus each subgroup treatment interaction term coefficient. Significance is then determined by the appropriate F-test of the hypothesis that this sum equals zero.

***Indicates that the impact differs statistically from zero at the 0.01 level.
**Indicates that the impact differs statistically from zero at the 0.05 level.
*Indicates that the impact differs statistically from zero at the 0.10 level.

##Indicates that the impact differs with respect to this characteristic at a 0.05 level of significance.

First-summer data on test score outcomes were also aggregated and analyzed to assess the average first-summer impact of STEP participation for all three cohorts. STEP participation increased reading scores by an average of 8.79 points and math scores by an average of 17.19 points. These impacts are equivalent to 0.4 of a grade in reading and 0.5 of a grade in math. Both estimates are statistically different from zero at the 0.01 level.

Impacts by Gender Subgroups

The results for Cohort III indicate that both males and females derived significant, statistically equal benefits from STEP participation in both reading and math. The net impact on reading scores for Cohort III females (8.8 points) was approximately the same as that observed for Cohort II in 1986. In math, the net impact for females was slightly less than that seen in Cohort II, but still quite substantial. For males, the net impacts in both reading and math were substantial and statistically significant, but approximately one-third smaller than those seen for Cohort II. Since treatment males in Cohort III actually performed better in reading and about the same in math as their Cohort II counterparts, the smaller impacts attributed to STEP participation are due primarily to smaller learning losses in the control group.

Impacts by Racial/Ethnic Subgroups

With the exception of those for Asian youth, the impacts observed within various racial/ethnic groups reflect the pattern for the overall sample, i.e., they were generally significant but smaller than those estimated for Cohort II. Among black treatment youth, the impact of STEP participation on both reading and math scores was statistically significant. Participation in STEP produced test scores that were approximately 11 points higher in reading and 17 points higher in math for treatment youth relative to controls.

For Hispanic youth, STEP participation resulted in a significant impact on math scores, but not on reading scores. These results are almost identical to those observed for Hispanic participants in Cohort II. In math, the net impact of 13 points is only slightly less than the 15-point impact observed for Cohort II Hispanics. The net impact on reading scores for Cohort III Hispanics was not statistically significant. However, the estimated impact of eight points in reading is not statistically different from the estimated impacts on reading scores for other racial/ethnic groups.

The magnitude of impacts on test scores was greatest for the racial/ethnic category composed of "whites and others," which includes Native Americans, Native Alaskans and so on. Treatments in this subgroup scored an average of 15 points higher in reading and 37 points higher in math than control youth. "Whites and others" is the only racial/ethnic category in which control losses were as large as those observed for previous cohorts; performance among treatment youth was stronger than seen in previous cohorts.

Cohort III Asians were the only racial/ethnic group that did not benefit from STEP participation in either reading or math. Although Asian treatment youth gained approximately 0.8 of a grade equivalent in reading over the summer, controls gained nearly as much--0.7 of a grade equivalent. This lack of impact on reading scores is consistent with the results for Cohort II. In math, however, the failure of Cohort III Asians to show a program impact contrasts with the substantial impacts seen for Cohort II Asians (almost 25 points, derived primarily from a 19-point increase in average treatment scores). We compared various baseline characteristics of Asian youth in Cohort III with those of Cohort II for a possible explanation.[46] However, we found no significant differences that could help to explain the differential impact across the two cohorts.

Thus, with the exception of those for Asian youth, first-summer impacts of participation in STEP among Cohort III youth were generally consistent with the impacts observed for the various racial/ethnic subgroups in Cohort II, though they were somewhat smaller in magnitude. Compared with results for Cohort I, the impacts observed for both Cohorts II and III were stronger among black and "white and other" youth, but smaller among Asian and Hispanic youth.

Impacts by Sites

In STEP's third summer, Cohort III treatment youth realized significant short-term impacts on MAT scores in four of the five sites. The net impact of participation on reading performance was statistically significant in three sites (Boston, San Diego, Seattle), while the net impact of STEP participation on math scores was significant in all sites except Fresno.

The net impacts observed in Boston and San Diego for Cohort III in both reading and math were slightly stronger than those observed for Cohort II. Estimated net impacts in reading were about 14 points for both sites; in math, the net impact of STEP participation was approximately 24 points in Boston and 19 points in San Diego. Youth in Seattle also showed significant impacts in both reading and math, but these impacts were smaller than those estimated for Cohort II by about half in reading and by two-thirds in math.

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[46] In addition to the characteristics we had examined for the overall sample (see Appendix C), we looked at the distribution of Asian subgroups for the two cohorts. We found no difference in the ethnic composition of the Asian samples of Cohorts II and III.

In Portland, Cohort III treatment youth improved their math scores relative to controls, but not their reading scores. While the math results are comparable to those observed for Cohort II, the lack of an impact in reading contrasts sharply with the significant impact observed in 1986. Two changes that occurred in the Portland program in 1987 might help to explain the differential impacts on reading scores for Cohorts II and III. First, in previous years the site had used one remediation teacher for each class; for Cohort III, youth were instructed by separate reading and math teachers. To accommodate this new teaching structure, students' schedules became rather complicated, often changing on both a daily and weekly basis. Of itself, however, this change probably does not account for the lack of impact on reading scores. In fact, one might expect youth to perform better when instruction is provided by specialists in reading and math. Also, the strong math impact suggests that the teaching structure and schedule were not necessarily detrimental.

A change that is more likely to have affected Cohort III reading scores in Portland was the introduction of a new supplemental reading program. Although the program chosen had proven successful in other settings, its approach was not well-integrated with the module portion of Practical Academics. Since most teachers were unfamiliar with this supplemental program, they were required to learn a new approach to teaching reading skills during implementation. As a consequence, reading instruction may have suffered.

The most disappointing and perplexing results were seen in Fresno. Participation in STEP resulted in no significant impact on either reading or math scores. The absence of a program impact on reading results is frustrating because Cohort III treatment youth gained about 0.6 of a grade level. But control youth gained about 0.4 of a grade equivalent over the summer, meaning that no impact on reading scores could be attributed to participation in STEP. The lack of a significant impact on Cohort III math scores is also due almost entirely to smaller losses among control youth.

A new test schedule may have contributed to better posttest performance by both control and treatment youth in Fresno. In the first summer of the demonstration, youth in Fresno had been posttested in afternoon sessions that followed a morning of either classes (for treatment youth) or work (for control youth). P/PV felt youth would perform better if they were tested in the morning and requested that site staff implement a change in schedule for Cohort III. The similar improvements in average posttest scores (compared with those for Cohort II) in both reading and math among both treatments and controls likely reflect this change.

While the schedule change may explain why Cohort III treatment youth have improved reading scores without a corresponding program impact, it cannot explain the disappearance of any STEP impact on math scores. If a higher percentage of control youth than treatment youth in Cohort III were placed in jobs that required use of basic skills, the control group's improved relative performance in math on the posttest might be expected. An examination of the types of jobs, however, indicates that control youth were slightly less likely to be placed in jobs requiring mostly "mental" skills. (See Chapter IV.)

Another explanation may be that Cohort III treatment youth may actually have gained less from STEP participation than Cohort II treatments, with their higher posttest scores being solely attributable to the change in testing schedule. Such a drastic reduction in treatment impacts would likely be due to some alteration in the implementation of the program. One change that occurred in Fresno was in the assignment of homework. In 1986, Cohort II treatment youth received and completed a considerable amount of optional homework during STEP. Cohort III youth, on the other hand, completed virtually no homework assignments; thus, they may have spent less time actually practicing the skills being taught in STEP.

Differences Within Sites

Remediation occurred at multiple sites in Boston and San Diego. We examined remediation differences between subsites of these sites by comparing changes in mean test scores from pretest to posttest and by using regression techniques. Net impacts estimated by the regression models for all remediation subsites are presented in Table VI.4.

San Diego reduced the number of high schools it used as remediation sites from five in 1985 and 1986 to two in 1987. In the first summer of STEP (1985), there were large and significant differences in pretest to posttest score changes among the five sites. In 1986, differences across subsites were smaller and not statistically significant. The increased consistency of scores during the second summer was likely due to a more even implementation of remediation across sites.

We expected that the reduction in the number of subsites in the third summer, in conjunction with a teacher corps consisting primarily of returning teachers, would further reduce subsite differences in San Diego. In addition, we expected that the more ethnically heterogeneous sites and classrooms that were a byproduct of the reduction in the number of sites would result in more consistent impacts. Thus, we were not surprised that the

Table VI.4

NET IMPACT OF PARTICIPATION IN STEP ON MAT SCALED SCORES IN READING AND MATH BY REMEDIATION SUBGROUP IN BOSTON AND SAN DIEGO COHORT III

Site	Reading Scores	Math Scores
Boston		##
UMass	22.58**	39.00***
Northeastern	2.87	2.54
BU	6.16	21.90
Harvard	8.91	9.86
	###	
DRP[a]	-10.76	9.76
Non-DRP	17.87***	30.56***
San Diego		
Gompers	13.77**	17.71**
Madison	12.30*	22.34**

[a] Refers to youth who received DRP (Degrees of Reading Power) test preparation instruction.

***Indicates that the impact differs statistically from zero at the 0.01 level.

**Indicates that the impact differs statistically from zero at the 0.05 level.

*Indicates that the impact differs statistically from zero at the 0.10 level.

###Indicates that the impact differs across these groups at a 0.01 level of significance.

##Indicates that the impact differs across these groups at a 0.05 level of significance.

estimated impacts of STEP participation at the two San Diego sites did not prove to be statistically different. In fact, the magnitudes of the estimated impacts, all of which are significant, are almost identical. It is clear from these results that the implementation of remediation in San Diego was very similar in the two subsites.

Of the four university sites Boston used during Summer 1987, three had been STEP sites since the pilot program in 1984 and one, Harvard, was used for the first time. Although there were significant differences in reading across the three universities in Boston in 1985, there were no significant differences in 1986.

Classroom observations in 1987 suggested that implementation of remediation was, not unexpectedly, more problematic in the new subsite than in the universities with three years of STEP experience. Problems associated with start-up may have been reflected in the quality of instruction and ultimately in the test score results.

It should be noted that each subsite averages only 31 students; thus, only very large differences are likely to be detected statistically. Still, the pattern of net impacts across subsites in Boston indicates an uneven implementation of the remediation component. As shown in Table VI.4, subsite impact estimates varied greatly. However, the variation is not due primarily to youth at Harvard performing less well, but to youth at the University of Massachusetts performing extremely well.

Pretest scores suggest that these results may reflect differential ability levels and needs of students as much as differential implementation of the program. On average, students attending the University of Massachusetts began the summer at a much higher level in both reading and math than youth at any of the other three sites. The average pretest reading score for UMass students was equivalent to a grade level of 7.4--more than a full grade level higher than the average for STEP youth at Harvard (6.2) and Boston University (5.7) and more than two grade levels higher than those at Northeastern (5.2). In math, the range of average pretest scores was smaller but still substantial, ranging from a high of 7.4 at UMass to a low of 5.9 at Northeastern. Thus, it appears that for Boston youth who entered the program with baseline skills at the fifth- or sixth-grade level the program did not affect test scores; for those who started near the seventh-grade level or higher, however, participation significantly affected test scores.[47]

Offering remediation at four different universities in 1987 was not the only potential source of differential impacts within the Boston site. The site also offered slightly different remediation programs to two distinct groups of students. The Boston Public Schools (BPS) require that students attain a minimum score of 52 on the Degrees of Reading Power (DRP) test in order to be promoted from the eighth to ninth grade. Approximately 40 of the Cohort III treatment youth participating in STEP (distributed approximately equally across the four universities) had not attained this minimum score; BPS agreed to allow these youth to retake the DRP at the conclusion of the summer program. As part of their STEP remediation classes, they participated in DRP tutoring during the skill reinforcement portion of the curriculum.[48]

Since these youth received a somewhat different skill reinforcement program, we tested whether the impact of participation varied according to whether youth participated in DRP tutoring sessions. For this analysis, we examined changes in mean test scores and estimated a regression model that provides the net impact of participation for these two groups of youth. The results of the regression analysis are presented in Table VI.4.

Results indicate that while STEP participation had no impact on the MAT test scores of the DRP youth; non-DRP youth improved their performance greatly. In fact, the differences in program impacts on reading scores for the two groups are statistically significant. Part of the explanation may lie in the degree to which these two subgroups are educationally deficient. Their average pretest scores are significantly different; youth in the non-DRP group scored, on average, at the 7.4 grade level in reading and the 7.7 grade level in math. Students in the DRP group, meanwhile had average pretest scores equivalent to the 3.7 grade level in reading and the 4.7 grade level in math.

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[47] That STEP participation has a greater impact on the basic skill levels of youth who enter the program at a higher level remains only a hypothesis, since we have no other evidence that baseline skill levels influence impacts on test scores. To test for differential impact due to pretest score, we estimated a model that included a pretest score by treatment status interaction term in the equation. (The coefficient was not significant.) However, because pretest score is highly correlated with the other variables in the model, this estimate is biased. To truly test whether the impact of STEP differs by ability level, one needs a measure of "innate ability" that is unrelated to such variables as grade in school or family size. Such a measure is not available.

[48] The four hours of skill reinforcement and SSR per week was presumably split between reinforcement of reading and math skills. Since the DRP concentrates on reading skills, the nine to 10 hours spent preparing for this test represented about 70 percent of the time available for reading skill reinforcement.

These average scores are not notable solely because they indicate statistical differences at baseline between the two groups. They also show that the average MAT reading score for the DRP group is below the fourth-grade floor required for STEP participation, a floor that exists to screen out youth who do not have the minimal decoding skills considered necessary to benefit from the STEP remediation curriculum. If pretest scores are an accurate reflection of reading skills, we believe the average DRP youth cannot be expected to improve on the basic skills measured by the MAT through STEP. Thus, we see these results as demonstrating the need to enforce a minimum skill level in reading for STEP participation.[49]

Impact of Teacher Characteristics

Classroom observations in STEP have indicated possible variation in the quality of instruction being delivered across sites and across classrooms. In looking at this issue, we collected personal information from remediation teachers to determine whether the characteristics of instructors affect outcome measures.

The STEP sites, assuming that participants would be more responsive to teachers from similar backgrounds, attempted to recruit a cadre of teachers from diverse racial/ethnic backgrounds. It was also expected that effective STEP instructors would have previous experience relevant to the situation they would face in STEP and would have an instructional style that is compatible with STEP's instructional approach. Thus, the personal information questionnaire completed by remediation teachers was designed to measure prior experience and compatibility with STEP's instructional approach, as well as collect basic demographic information.

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[49] Although the MAT outcomes indicate that the basic skills measured by that test were not affected by STEP participation, many DRP-tutored youth were able to improve their DRP scores. According to the Boston Case Study, almost 90 percent of the youth who received DRP tutoring raised their DRP scores, with approximately 60 percent achieving at least the minimum score of 52 required for promotion.

To examine whether teacher characteristics affected performance, we included a dummy variable for each teacher in the regression models. Results indicate that a teachers' background caused no significant difference in treatment youth's performance in either reading or math.[50] Although observational data suggest that there were variations in the quality of instruction received by STEP youth, these variations appear to have had no impact on MAT scores.[51]

Attitudes Toward Education

One might hypothesize that a program that increases an individual's skills in reading and math might also influence attitudes toward school, particularly toward reading and math. If a youth perceives that he or she can be successful in school, his or her expectations and desire for educational attainment may increase. Conversely, one might also theorize that attitudes toward school may decline during a summer program, given that youth have been engaged in reading and math activities during time that would ordinarily have provided a break from the regular school year. Measures of STEP participants' enjoyment of reading and math and their expectations for higher education were collected at baseline and endline to investigate these hypotheses.

Students were asked to rate their enjoyment of reading and math using five response categories, ranging from "very much" (coded as 5) to "not at all" (coded as 1). (Thus, a higher number connotes greater enjoyment.) For both treatment and control youth in Cohort III, the mean level of enjoyment of reading at baseline was approximately 3.2, while the mean for enjoyment of math was about 3.5. The mean level for enjoyment of reading did not change significantly over the summer for either group, and there was no treatment/control difference in the amount of change that did occur. The mean level for enjoyment of math, however, decreased significantly among treatment youth. This decrease was also significantly different from controls, whose mean enjoyment did not change. These results were further confirmed by the multivariate analyses. Thus, while participation in STEP did not affect enjoyment of reading, it had a negative impact on enjoyment of math.[52]

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[50] Since this analysis uncovered no differences in outcomes across teachers, no further analysis of teacher characteristics with regard to learning outcomes was conducted.

[51] The issue of quality of instruction and teacher characteristics will be discussed further with respect to life skills outcomes.

Educational expectations were measured by asking youth the highest grade in school or year in college they expected to complete. To examine whether these expectations changed as a result of STEP, we first compared the percentage of youth who expected any post-high school education at baseline with the corresponding percentage at the end of the summer. At baseline, 82 percent of treatment youth and 85 percent of control youth indicated that they expected to continue their education after high school. These percentages decreased slightly for both groups over the summer--by four percentage points for treatment youth and five percentage points for control youth. These decreases are not statistically significant, and the decrease for treatments is not significantly different from the decrease for control youth.

Two multivariate models for examining the impact of STEP on educational expectations were estimated. The first model provided an estimate of the probability that a youth would report expecting to go on to post-high school education. The second model regresses an ordinal measure of youth's self-reported educational expectations[53] at the end of the summer on treatment status, baseline educational expectations and the set of explanatory variables included in previous learning analysis models. Results of both analyses indicate that participation in one summer of STEP had no impact on educational expectations.

One limit on these models is that STEP youth generally seem to have unrealistically high educational expectations, given the dropout rates for at-risk youth. One might theorize that participation in STEP would increase educational expectations, but the high baseline levels leave little room to detect such improvement. Conversely, one might expect that treatment youth would develop more realistic (i.e., lower) expectations as a result of STEP participation. It seems unlikely, however, that such a change would occur over the short duration of the summer program; thus, the lack of short-term impacts is not surprising. Any impacts STEP participation has on educational expectations are not likely to appear until a later point in a youth's educational career.

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[52] One remediation consultant suggests that this result is not unexpected, since a common complaint among students in the program is that they have to work too hard.

[53] This measure of educational expectations ranges from less than high school graduation (which is coded as 1) to more than four years of postsecondary education (coded as 7). Thus, an expectation to graduate from high school is coded as 2, one year of college is coded as 3, and so forth through four years of college.

School Reenrollment Information

The final educational outcome measure we examined was the STEP sample's reenrollment rate for the 1987-88 school year. The number of youth in the Cohort III sample who were attending school at the beginning of that school year was obtained from school district records. The percentages of Cohort III treatment and control youth who returned to school in the fall are presented in Table VI.5. In general, the percentage of youth who returned to school was high--more than 94 percent for both treatment and control youth. That such a high percentage returned to school is not surprising, given that virtually all of these youth were younger than 16, the usual age at which youth are legally able to leave school.

To determine whether STEP participation affected reenrollment, we tested whether reenrollment rates differed by treatment status. For the overall sample of Cohort III youth, no significant difference in the percentage who returned to school was found using either difference-in-means or logit analysis. In addition, there was no difference by treatment status in the percentage of youth who returned to school for any subgroup. In every subgroup, more than 90 percent of both control and treatment youth returned to school.

Summary: Academic Performance Outcomes

The impact of STEP on learning, as measured by posttest MAT scores, was generally positive and strong for Cohort III. The net impacts of participation for Cohort III treatments were 9.33 points in reading and 16.43 points in math. These impacts are equivalent to 0.5 of a grade in reading and 0.6 of a grade in math.

These learning impacts for Cohort III are almost as strong and as widespread as those measured for Cohort II following its first summer of STEP. In math, impact analyses showed that Cohort III treatment youth of both sexes, from four of the five demonstration sites, and from all racial/ethnic groups except Asians benefited from STEP participation.

In reading, significant benefits of participation were seen for both sexes; for black and "white and other" youth; and for youth in three of the five sites. Impacts for Hispanics and Asians, and for youth in Fresno and Portland, however, were not significant. Teacher characteristics showed no significant influence on posttest scores in either reading or math.

Table VI.5

COHORT III YOUTH WHO REENROLLED IN SCHOOL, FALL 1986, OVERALL AND BY GENDER, RACE/ETHNICITY AND SITE

Characteristic	Treatment	Control	Difference
Overall	94.7%	94.6%	0.1%
Gender			
Male	95.0%	92.9%	2.1%
Female	94.4	96.3	-1.9
Race/Ethnicity			
Asian	97.6%	99.0%	-1.4%
Black	93.7	95.6	-1.9
Hispanic	95.9	92.5	3.4
White & Other	92.0	87.8	4.2
Site			
Boston	95.2%	97.5%	-2.3%
Fresno	94.6	93.5	1.1
Portland	91.9	90.4	1.5
San Diego	94.7	97.6	-2.9
Seattle	97.4	94.4	3.0

Impacts of STEP on enjoyment of reading and math, educational expectations and rate of returning to school were also examined; most outcomes were neither positively nor negatively affected by treatment status. The only exception was an impact on the self-reported enjoyment level of math, which declined over the summer for treatments, but did not change for control youth. However, the mean levels of enjoyment of reading among both treatment and control youth did not change over the summer. A slight decrease was also seen in the percentage of treatment youth expecting post-high school education, but was not significant. Finally, there was no discernible effect of STEP on return to school rates for SY 1987-88. Nearly 95 percent of both treatment and control youth reenrolled in school.

LIFE SKILLS OUTCOMES (LSO)

By improving the level of youth's basic skills, STEP takes on one of the major factors related to high school dropout rates. The life skills component of STEP addresses another major factor--adolescent parenthood. The focus of the life skills component is the relationship between social and sexual decisions youth make at age 14 or 15 and their future experiences in education, the workplace and the family. In the long run, STEP aims to reduce teenage pregnancy and parenting rates. Impacts on these outcomes are difficult to detect at the end of the first summer, however, and the quest for useful measures will necessitate a longer follow-up period. Some short-term impacts can be detected over one summer of STEP, however, such as changes in treatments' knowledge of the consequences of teenage parenting, level of sexual activity, and knowledge about and use of contraceptives. The sections that follow present the findings of analyses of the first-summer fertility-related outcomes for Cohort III and the aggregate sample (Cohorts II and III) of STEP youth.

The Cohort III life skills analysis sample, used for estimating impacts on fertility-related knowledge, included 1,228 youth who were not missing information on any fertility-related variables.[54]

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[54] The variables included in the life skills multivariate analysis models are: treatment status, gender, age, and race/ethnicity, as well as baseline measures of family size, birth order, English language problems, absenteeism, knowledge of contraception, knowledge of availability of contraception, knowledge of the consequences of adolescent pregnancy, and responsibility for birth control. Demonstration site dummy variables were also included. In addition, dummy variables for measures of female-headed household, ever had sex (at baseline) and ever had a sex education course were included. Dummy variables for non-response on these three measures were also included in order to limit sample attrition.

In this sample, gender, age and racial/ethnic compositions were not significantly different between the treatment and control groups. A logit model was estimated to investigate the possibility of differential attrition by treatment group status. The results indicate that treatment/control group status did affect the probability of youth from the original randomized sample being in the life skills analysis sample. Controlling for other factors, treatment youth were 8 percent more likely to be in the life skills analysis sample. That is, treatment youth were more likely to have taken the posttest and/or have answered all questions related to the life skills analysis. Thus, a correction factor was estimated and was included in all multivariate models that used the learning analysis sample. With the inclusion of this correction factor, we are confident that the estimated impacts are unbiased, consistent indicators of the treatment effect on outcomes for the life skills analysis sample.[55]

Knowledge of Contraception

To assist youth in making responsible social and sexual decisions, a portion of the life skills curriculum concentrates on the presentation of factual information related to avoidance of pregnancy. A visit to a comprehensive health facility is included as part of the curriculum to heighten awareness of the accessibility of health care and family planning services.

We hypothesized that participation in the life skills classes would result in increased knowledge of contraception over the course of the summer program. To test this hypothesis, we developed two means of measuring such knowledge at baseline and at the end of the summer. The first was a test consisting of 12 statements about various methods of contraception that youth must judge to be true or false. The mean number of correct responses to the statements at baseline (presented in Appendix Table B.11) demonstrates the need these youth have for birth control information; both treatment and control youth in Cohort III were able to correctly answer an average of only five of the 12 statements at baseline.

After the posttest, the impact of STEP on knowledge of birth control was estimated using a multiple regression model. The results, which are presented in Table VI.6, indicate that treatment youth significantly increased their contraceptive knowledge over the summer by an average of 2.99 questions more than control youth, whose knowledge did not increase. This represents an improvement of more than 50 percent over the mean baseline level.

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[55] The construction of the correction factor was discussed in Chapter III.

Table VI.6

NET IMPACT OF PARTICIPATION IN STEP ON BIRTH CONTROL KNOWLEDGE VARIABLES FOR COHORT III BY GENDER, RACE/ETHNICITY AND SITE

	Cohort III		
Group	Knowledge of Contraception[a]	Knowledge of the Availability of Contraception[b]	Knowledge of the Consequences of Teen Pregnancy[c]
Overall	2.99***	2.09***	0.22***
Gender	##		
Male	2.76***	2.00***	0.18***
Female	3.35***	2.23***	0.28***
Race/Ethnicity	###	###	#
Asian	3.87***	3.00***	0.36***
Black	2.80***	1.91***	0.24***
Hispanic	2.76***	1.91***	0.08
White & Other	2.88***	1.79***	0.14
Site	###	###	###
Boston	2.23***	1.55***	0.07
Fresno	2.71***	1.95***	-0.01
Portland	2.46***	1.64***	0.06
San Diego	4.94***	3.50***	0.69***
Seattle	2.61***	1.81***	0.28***

Note: The sample size for this analysis is 1,228. Impacts for each subgroup are computed as the sum of the treatment coefficient plus each subgroup treatment interaction term coefficient. Significance is then determined by the appropriate F-test of the hypothesis that this sum equals zero.

[a] Mean number of correct responses on a 12-item scale.
[b] Mean number of correct responses on an 8-item scale.
[c] Mean number of correct responses on a 2-item scale.

***Indicates that the impact differs statistically from zero at the 0.01 level.

###Indicates that the impact differs with respect to this characteristic at a 0.01 level of significance.
##Indicates that the impact differs with respect to this characteristic at a 0.05 level of significance.
#Indicates that the impact differs with respect to this characteristic at a 0.10 level of significance.

The second test of the impact of STEP on knowledge of contraception consists of participants' responses concerning the availability of eight birth control methods. Youth were asked to indicate whether each method is available without seeing a doctor. On average, Cohort III youth were able to correctly answer only three of these eight questions at baseline.

The results of the multivariate analysis, presented in Table VI.6 also indicate that STEP participation significantly increased youth's knowledge of the availability of birth control. At the end of the summer, treatment youth correctly answered an average of 2.09 questions more than controls, whose performance did not change. Given that the mean level at baseline was three, this outcome represents a 63 percent increase in knowledge of the availability of contraception.

The significant impact of STEP on knowledge of contraceptive methods and knowledge of contraception availability observed in the overall sample was also seen for both genders, for all racial/ethnic groups and across all sites. The magnitudes of the impacts are similar for males and females, and for black, Hispanic and white youth; however, they are stronger for Asian youth. This finding may be partly due to the relatively low level of contraceptive knowledge possessed by Asian youth at baseline. While the average youth in the STEP sample could correctly answer five of the 12 knowledge questions and three of the eight availability questions at baseline, Asian youth correctly answered an average of only three of the 12 knowledge questions and two of the eight availability questions. Thus, Asian youth had more room to improve their knowledge than did other racial/ethnic groups in the sample.

The impacts on knowledge of contraception for Cohort III were very similar in Boston, Fresno, Portland and Seattle; however, impacts in San Diego were considerably larger than those estimated for other sites. The magnitude of impacts on knowledge of contraception are stronger than those observed for Cohort II in every site except Portland and the range of impacts among the sites is far narrower than that observed for Cohort II.[56]

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[56] On the knowledge of contraceptive methods measure, the Cohort III range is 2.71--from 2.23 in Boston to 4.94 in San Diego. This compares with a range of 3.51 for Cohort II--from a low of 1.24 in Boston to a high of 4.75 in San Diego. For the impact on the knowledge of birth control availability measure, the across-site range was 2.68 for the second cohort (the low, in Boston, was 0.63 while the high, in San Diego was 3.31). The comparable range for the third cohort is 1.95; Boston again had the smallest impact, 1.55, and San Diego again had the largest, 3.50.

Cohort III improvements and the narrowing of previously divergent impacts may be due to increased experience of LSO instructors and better selection of new instructors across sites. Still, observations showed the LSO component to be at its best in San Diego, where two of the three life skills instructors for Cohort III also taught the LSO component of STEP during the first two demonstration summers. Furthermore, all three counselors had many years of teaching experience, including experience in both sex and drug education. Based on classroom observations, these San Diego instructors were outstanding. The high quality of instruction observed in the San Diego program may explain the considerably larger impact measured in that site.

Experience among instructors in the other sites was mixed. In Boston, five of the six Cohort III LSO instructors were teachers or had teaching experience; three had experience teaching health or sex education and two taught LSO during the 1985 and/or 1986 summers.[57] Cohort III LSO instructors in Fresno and Portland were all teachers with no previous LSO experience, though three of the four had some previous health or sex education experience. One Seattle instructor was a teacher with previous health and sex education experience and two years of LSO experience; the other instructor, however, had no previous sex education or LSO experience. This variation in the characteristics of instructors may have contributed to the site-by-site variations in impacts.

To further investigate the possibility that instructor characteristics affected life skills outcomes, a dummy variable for each instructor was included in the regression models. Results from these regressions indicated that there were indeed differences in impacts across LSO instructors. After determining that teachers make a difference in the observed impacts, we added measures of several teacher characteristics to the models--namely, the race/ethnicity and gender of the instructors, their years of teaching experience, whether they are health/sex education teachers, whether they had taught LSO during the first and/or second summers of STEP, their average weekly preparation time and whether they had prior experience with a special needs population. The regression analyses indicated, however, that none of these characteristics affected the impact of STEP on knowledge of contraception or the availability of contraception. Although observational data suggested variation in the delivery of the LSO curriculum, measured teacher characteristics do not capture the critical differences that were observed across sites.

Analyses of the impact of STEP on the two measures of knowledge of contraception were also completed for the aggregate sample of STEP youth in Cohorts II and III (the results are presented in Appendix Table B.12). Again, the impacts are significant overall and for each of the subgroups. As was true for the learning analyses, the aggregate results represent an average of the impacts observed for each of the two cohorts. Since the Cohort III results were generally a little stronger than those for Cohort II, the aggregate coefficients are slightly smaller than those for Cohort III.

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[57] These teachers were assisted by two health/sex educators.

A final indicator of the impact of STEP on fertility-related knowledge was the participants' understanding of the consequences of adolescent pregnancy. Cohort III youth were asked two multiple choice questions--the first concerning whether girls who became mothers were likely to continue their education or drop out of school, and the second concerning average cost of pregnancy and the first year of a baby's life. At baseline, Cohort III youth averaged less than one correct answer for the two questions. Treatment youth significantly increased their understanding of these consequences over the summer, doing so to a greater extent than control youth. The net impact of STEP on this fertility-related measure is reported in Table VI.6. For Cohort III youth overall, the estimated impact is 0.22. The impact on this outcome, however, is less consistent than that seen for knowledge of contraception. Although the impact for both males and females was significant, impacts among other subgroups were significant only for Asians, blacks, and youth in San Diego and Seattle. The inconsistency of impacts with regard to this measure may reflect the fact that information on costs associated with having a child are presented in an activity that is optional in the LSO curriculum.

Reported Sexual Behavior

Given the short duration of STEP and the timing of the sessions that deal with sexual behavior, we do not expect any impact on sexual behavior to be detectable at the conclusion of the first summer. Results of the logit analyses support this expectation. Two measures of reported sexual activity were examined--sexual experience (whether the youth had ever had sex) and recent sexual activity (whether the youth had had sex in the previous two months). Of the Cohort III youth who answered the sexual experience question at both measurement points, 41 percent of treatment youth and 48 percent of control youth indicated that they were sexually experienced at baseline, as shown in Table VI.7. Among those who had ever had sex, approximately 53 percent of both treatments and controls indicated that they had been sexually active within the previous two months.

After the summer, the percentage of both control youth and treatment youth reporting that they are sexually active increased significantly--by 6 percentage points and 8.3 percentage points, respectively. There was no significant difference among treatment and control youth on this measure. The percentage of sex-

Table VI.7

BASELINE TO ENDLINE CHANGES IN THE PERCENTAGE OF COHORT III YOUTH WITH SEXUAL EXPERIENCE, THOSE HAVING SEX IN THE PREVIOUS TWO MONTHS, AND USING CONTRACEPTIVES AT LAST INTERCOURSE

	Sexual Experience[a]		Sexually Active[b]		Contraceptive Use[c]	
	Baseline	Change Over Summer	Baseline	Change Over Summer	Baseline	Change Over Summer
Treatments	41.4%	8.3***	52.9%	5.4*	39.0%	1.0
Controls	47.9	6.0***	53.5	8.3***	35.7	-0.4
Difference		2.3		-2.9		1.4
Males						
Treatment	57.6%	9.3***	51.4%	7.9	38.7%	-1.5
Control	65.5	6.9***	52.6	10.4	33.1	1.8
Difference		2.4		-2.5		-3.3
Females						
Treatment	28.2%	7.4***	50.6%	6.1	39.5%	4.9
Control	29.9	5.0***	55.6	3.7	41.0	-4.9
Difference		2.4		2.4		9.8**

[a] Percentage of youth reporting that they have ever had sex (n = 608 treatments, 568 controls).

[b] Percentage of sexually experienced youth reporting that they have had sex in the previous two months (n = 223 treatments, 254 controls).

[c] Percentage of sexually experienced (at baseline) youth reporting use of contraception the last time they had sex (n = 218 treatments, 258 controls).

***Indicates that the change differs statistically from zero at the 0.01 level.
**Indicates that the change differs statistically from zero at the 0.05 level.
*Indicates that the change differs statistically from zero at the 0.10 level.

ually experienced youth reporting recent activity also rose significantly for both groups. The significant increases are not surprising, given the effects of a summer's passage for this age group. Treatments rose only 5.4 percentage points, in comparison with 8.3 percentage points for controls; however, the difference was not statistically significant.

Estimates of the impact of STEP participation on either measure were not statistically significant for either the Cohort III sample or the aggregate sample of Cohorts II and III. Thus, STEP participation had no measurable effect on reported sexual activity during the first summer. Whether participation in STEP will affect reported sexual behavior in the long term cannot be determined until analyses of follow-up interview data are completed.

The final fertility-related measure of the impact of STEP participation for Cohort III youth is change in reported contraceptive use. Of the 1,228 Cohort III youth in the life skills analysis sample, only those youth who reported previous sexual experience (530 youth) were asked the questions that dealt with contraceptive use. Thus, the sample available for analyzing contraceptive use was smaller than that used in the previous analyses.[58] Of the Cohort III youth who were sexually experienced, 90 percent (476 youth) responded to the contraceptive use question at both baseline and endline.

As shown in Table VI.7, among Cohort III youth who responded to the contraceptive use questions, 39 percent of treatments and 35.7 percent of controls indicated at baseline that they had used some form of birth control the last time they had sex. At the end of the summer, only 35.3 percent of controls reported using some form of contraception during their most recent intercourse, while 40 percent of treatments did so. Thus, for treatment youth, use of contraception changed by 1.4 percentage points more than for control youth.

Logit analysis of the probability of reporting use of contraception during last intercourse indicated no significant impact of STEP participation on Cohort III youth who were sexually active during the program period. Treatments were only 7.3 percent more likely to report having used contraception during their last intercourse than controls. This is much smaller than the impact on reported contraceptive use observed among Cohort II treatments, who were 53 percent more likely than controls to report having used contraception during their most recent intercourse.

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[58] The combined sample of Cohorts II and III, however, contains 1,019 youth with previous sexual experience. This larger sample allows us to examine impacts on subgroups as well as for the overall sample. Some of the discussion presented below focuses on outcomes for the aggregate sample rather than for Cohort III.

The lack of a significant impact hides opposite trends in reported contraceptive use for males and females in Cohort III. The changes in the percentage of youth reporting contraceptive use shown in Table VI.7 indicate that contraceptive use decreased slightly among treatment males and increased among control males. Among females, however, contraceptive use increased for treatments and decreased for controls. The results of the logit analysis shown in Table VI.8 also indicate opposite effects for males and females.[59]

The results for the aggregate sample of youth from Cohorts II and III suggest that participation in STEP does affect reported contraceptive use. Among sexually active youth, treatments were 26.8 percent more likely to report having used contraception during their last intercourse than control youth.[60] In the aggregate sample, however, there was no difference between males and females in the estimated probability of reported contraceptive use. Male treatment youth were 27.6 percent more likely than male control youth to report contraceptive use during their most recent intercourse; among females, treatment youth were 24.8 percent more likely to report use of contraception. In Boston, San Diego and Seattle, sexually active treatment youth were significantly more likely than controls to report having used contraception during their most recent intercourse--by 50 to 60 percent in Boston and San Diego, and 25 percent in Seattle. In Fresno and Portland, there was no statistically significant impact on reported contraceptive use.

Summary: Life Skills Outcomes

Participation in STEP clearly influenced the fertility-related knowledge of youth. With respect to knowledge of contraceptive methods, treatment youth correctly answered an average of 2.99 questions more than controls at the end of the summer. Similarly, treatment youth increased their knowledge of the availability of contraception by an average score of 2.09 questions more than control youth. These outcomes represent increases of approximately 50 to 65 percent over treatments' mean baseline

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[59] As with the contraceptive knowledge outcome variables, we tested whether LSO teacher variables influenced reported contraceptive use. There was no significant difference in the impact of participation in STEP on reported contraceptive use due to LSO instructors.

[60] Since these aggregate results represent an average of the impacts for both cohorts, this significant impact is likely due primarily to the strong impact for Cohort II.

Table VI.8

ESTIMATED IMPACTS OF STEP ON CONTRACEPTIVE USE AMONG SEXUALLY ACTIVE YOUTH IN COHORT III AND THE AGGREGATE SAMPLE OF COHORTS II AND III[a]

	Cohort III	Aggregate
Overall	7.3%	26.8%***
Gender		
Male	-38.7%	27.6%**
Female	39.1	24.8
Site		
Boston	n.a.	50.6%**
Fresno	n.a.	-25.8
Portland	n.a.	20.2
San Diego	n.a.	57.2 ***
Seattle	n.a.	25.3 *

n.a. = not available.

[a] Impacts are estimated using logit regression for the sample of youth (Cohort III = 329, aggregate = 645) who indicated at the end of the summer that they had been sexually active in the previous two months. The impact is the probability that treatment youth used contraception at most recent intercourse relative to the probability for controls. (Thus, aggregate treatment youth were 26.8% more likely than controls to report use of contraception).

***Indicates that the impact differs statistically from zero at the 0.01 level.

**Indicates that the impact differs statistically from zero at the 0.05 level.

*Indicates that the impact differs statistically from zero at the 0.10 level.

scores. Understanding of the consequences of adolescent pregnancy also increased among treatment youth in Cohort III.

Treatment males and females from all racial/ethnic groups and all five sites significantly improved their knowledge relative to control youth on the two measures of fertility-related knowledge. These results for Cohort III are even stronger than those seen for Cohort II youth, who showed an increase in knowledge of contraception of approximately 50 percent.

Although both classroom observations and statistical analysis indicated that some instructors are better at teaching the LSO curriculum than others, none of the measured characteristics (such as race/ethnicity or previous experience) affected the outcome measures of fertility-related knowledge.

From the analysis of the first summer for Cohorts II and III, we concluded that STEP participation had no short-term impact on reported sexual behavior. The major short-term impact of STEP is an increase in the reported use of contraception among the aggregate sample of Cohorts II and III youth who were sexually active during the summer.

VII. SCHOOL-YEAR AND SECOND-SUMMER IMPACTS FOR COHORT II

Cohort II youth were offered school-year support (SYS) activities during SY 1986-87 and many returned for their second summer of STEP instruction and work experience. In this chapter, the effects of the first summer of STEP on the behavior of treatment and control youth during the intervening school year are examined. These school data analyses are considered estimates of mid-program impacts.

We also examined which Cohort II youth returned for a second summer of STEP and how their performance had changed by the end of that summer. By analyzing end-of-program impacts for youth who participated in STEP for a full 15 months, as evidenced by outcomes measured at endline 1987 or during follow-up interviews, we were able to determine how reading, math and life skills outcomes for Cohort II treatment youth had changed over the 15 months. MAT scores were the primary source of reading and math data. Information on sexual behavior and contraceptive use was collected from Cohort II youth in follow-up interviews conducted soon after they completed STEP's second summer (Fall 1987).

SCHOOL DATA ANALYSIS

STEP was designed to improve students' school performance by providing them with needed remediation, increasing their understanding of the consequences of not finishing school, giving them information on how to prevent pregnancy and possibly helping them have a positive school experience. To measure STEP's short-term impact on school performance, five outcome measures for the academic year 1986-87 were examined: attendance, test scores, credits earned, grade promotion and dropout behavior.[61]

Overall, STEP participation showed no effect on the behavior of Cohort II youth during the school year immediately following their first summer in STEP (1986-87). However, there may be program impacts on school performance in future years. It is also important to note that Cohort II youth had yet to participate in a second summer of STEP.[62] Before presenting results for Cohort II in greater detail, issues that indicate a need for cautious interpretation must be addressed.

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[61] One school outcome that was not considered is grade point average, which is too difficult to interpret without information on the content and difficulty of courses taken. Without adjusting grade point average for such factors, perverse results are possible. If, for instance, STEP treatment youth took college preparatory courses while controls did not, it might appear that STEP had a detrimental effect on grade point averages.

Data Limitations

No one measure provides a complete picture of school performance, and no one measure is without limitations. Thus, to investigate the effect of STEP participation on all five school performance outcomes, we looked for a confluence of impact results that would give us greater confidence in our conclusions. A brief summary of the issues encountered in analyzing each outcome is presented in this section.

Attendance

Data on absenteeism are notoriously inaccurate. For example, only 54 percent of the records kept for Cohort II youth in the Boston school district show that the sum of the recorded number of days present and days absent equal the total number of days in the school year. In fact, 20 percent of the records show attendance figures that do not fall within plus or minus 10 days of the true school year. When measurement errors are this large, the effect of the program on attendance must be quite substantial in order to be statistically significant.

Problems also occur when data collection varies systematically across sites. Differences in attendance-taking practices led us to suspect that the accuracy of data differed. One key variation was noted in attendance data collection. Schools in Portland took attendance in each of eight periods during the day; all other site school districts took attendance only in homeroom.[63] There was also a key difference in the method of recording attendance. In Boston, teachers recorded a student's presence or absence; elsewhere, only absences were recorded.

Although differences in attendance-taking practices led us to interpret cross-site findings cautiously, steps were taken to minimize the problem. Systematic errors across sites are adjusted by analyzing attendance in a regression setting where specific site influences are controlled statistically.[64] The robustness of the pooled site results were also checked by examining absenteeism separately by site.

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[62] Analysis of impacts for Cohort I, presented in the next chapter, provide evidence that STEP may have an effect on school performance and behavior in the following year--i.e., the first postprogram year.

[63] To make Portland attendance data comparable to that of the other sites and to be able to pool data across sites for the analysis, we used attendance during the first period of the day.

Standardized Test Scores

Within a given school district, analysis of test score data was straightforward. However, sites administered different standardized tests, and the scores they obtained could not be compared directly.

To conduct a cross-site analysis, test scores were converted to "normal curve equivalents" (NCEs). NCEs are similar to test score percentiles in that they range from 0 to 100; unlike percentiles, however, they are a linear measure that can be subjected to regression analysis. The transformation of the scaled scores to normal curve equivalents enabled us to pool test data across sites and analyze test data for the entire sample. Analyzing a larger sample size also increased the ability to detect small program impacts.

To verify the accuracy of the impacts estimated from the pooled analysis of test-score NCEs, site-by-site analyses of scaled scores were conducted. The analyses are relatively uncomplicated, but the sample within each site was relatively small, averaging approximately 150 treatments and 150 controls. Thus, it was difficult to observe statistically significant effects.

In addition, San Diego and Portland presented site-specific data collection problems. In San Diego, students are tested only every other year; thus, only sample members who were in the seventh, ninth or 11th grades were tested. Although this selection process did not bias the results presented in this report, it reduced the number of sample members for whom there were data. Therefore, an alternate method of determining the effect of STEP on reading and math skills in San Diego was used. We compared the treatment and control scores on the tests that Cohort II youth took at the beginning of their second summer of STEP. While a greater percentage of the sample took these tests than took the in-school standardized test, this approach has two disadvantages. First, since participants took the STEP test at the end of June and the in-school tests in April, STEP MAT test results are not entirely comparable to those on in-school tests. Second, because only some sample members returned for a second summer of STEP, those tested may not represent the full cohort; there might also have been a differential pattern of attrition for treatment and control group members, which could bias the impact estimates. Thus, differences in the test scores were examined only for returning treatments and controls, allowing us to better investigate the robustness of our in-school test results.

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[64] Another problem occurs if the degree of random measurement error varies significantly among the sites. The presence of measurement error makes an estimated impact appear to be closer to zero than it actually is. If the degree of the measurement error varied among the sites, the site impacts would be biased by different amounts. Thus, differences in the site impacts would not only indicate differences in the STEP impact by site, they would also reflect the differences in measurement error. It is not possible to ascertain with certainty whether the degree of measurement error varied by site. However, we found the variance in absenteeism was similar among all sites except Seattle, where the variance was 30 percent smaller than that seen in the other sites. Therefore, it may be easier to detect impacts, if they occurred, in Seattle than in the other sites.

In Portland, not all sample members took a standardized test, since Portland students are only tested annually between grades three and eight. Between the ninth and 12th grades, only students who have not yet met a minimum level of competency in reading or math are tested. This procedure provided test scores for approximately 66 percent of the Portland sample members. Thus, in Portland, we could address the question of how STEP affected the reading and math skills of those who have not already reached the minimum level of competency, or are in the eighth grade or below. However, the impact on test scores for the average Portland sample member could not be estimated.[65] Thus, the aggregation of the test scores across sites (using NCEs) excludes Portland.

Credits

As a third measure of STEP's impact on school-year progress, data were collected on the number of high school credits earned by each student. Because students who were in eighth grade or below had not yet earned high school credits, they were excluded from this analysis. This decreased the sample size for the credit accumulation analysis by 11 percent (to an N of 1,044), slightly decreasing the likelihood of finding statistically significant impacts.

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[65] This sample selection factor will not bias the school-year analysis for Cohort II this year because whether an individual has reached the minimum level of competency as of the school year after their first summer of STEP is independent of STEP. However, the level of a youth's math and reading skills is probably not independent of his/her STEP participation in any year after the first. Thus, next year we must correct for the selection process statistically or reframe the question as: given that a student had not met the minimum required level of competency as of the intervening school year, how many years does it take to achieve the competency level for STEP treatments versus controls?

Grade Progression

The fourth measure of school-year performance is grade progression, which is influenced by a school district's philosophy of grade promotion. Promotion in one school district may not signify the same academic advancement as promotion in another. Therefore, grade promotion measures a student's progress through a school system, not necessarily his/her skills acquisition or academic achievement.[66]

The more lenient a district's promotional philosophy, the smaller the estimated STEP impact is likely to be. At the extreme, suppose all students were promoted no matter what academic level they attained by the end of the year. There would be no difference in the proportion of treatments and controls who passed; therefore, the estimate would indicate that STEP had no impact on grade progression. Such an outcome, however, would not necessarily imply that STEP did not affect academic achievement.

Thus, caution must be used when comparing grade progression impacts within and across sites. This problem was minimized by using regression analysis and checking pooled results by analyzing promotion at each site separately. Nevertheless, the assessment of STEP's effect on educational progress must be based on analyses of both accumulated credits and grade promotion.

Dropout Behavior

Accurate statistics on dropout rates are very difficult to obtain. Schools often cannot identify students that have dropped out because they did not or, if under 16 years old, legally could not formally withdraw. Thus, most school districts remove students from the active enrollment list based on non-attendance. However, the manner in which school districts record such withdrawals varies considerably. In addition, since schools often qualify for funds based on the number of students they serve, the accuracy of dropout and enrollment data can be suspect.

To examine dropout behavior while recognizing these problems, we used two definitions of dropping out. Under the first definition, an individual was deemed to have dropped out if the school district so reported or P/PV so discovered.[67] Withdrawal because of expulsion was not counted as dropping out; only voluntary withdrawal, including withdrawal for such reasons as enlistment in the armed forces, was. The measures used to identify dropouts under the first definition are likely to underestimate the number of dropouts for two reasons. First, a school system will often report inaccurately that a student is still enrolled. Second, some youth whose records look suspicious were not located by P/PV. Some of these may be dropouts.

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[66] Only two sites, Boston and Seattle, reported promotion status as of the end of school year 1986-87. The proportion of students who are promoted differed substantially between these two sites. Among the controls, 81.3 percent passed in Boston and 98.3 percent passed in Seattle.

The second definition used took into account all youth identified as dropouts by the first definition, but also counted individuals for whom there was no apparent or identifiable reason for withdrawal. This measure of dropping out probably overstates the number who dropped out because some individuals could have transferred to other school districts without informing their previous schools. Thus, although neither definition provides an accurate gauge of the size of the problem, they do establish boundaries for a true measure. If measures based on these definitions reflect STEP's impact--measured as the difference in dropout behavior between treatments and controls--potential inaccuracies in the dropout rate may not be as large a problem as they first appear.

Findings from the analyses are presented in the next section. Impacts may be stronger in future years, since Cohort II youth still had their second summer of STEP participation to complete. The school-year results presented in the following section are essentially mid-program results.

Results

In this section, the impact of STEP on attendance, reading and math test scores, credits earned, grade promotion and dropout behavior during the school year immediately following the first summer of participation is examined for Cohort II youth. The discussion focuses primarily on estimates derived from regression analyses that pooled data from all sites. The results of site-by-site analyses conducted on these outcomes (presented in

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[67] P/PV attempted to contact any student for whom school data were missing by contacting the student's original school and using other local contacts, such as the student's school-year support mentor. Through these contacts, we sometimes learned that a student had dropped out. When enrollment in new school districts was reported, P/PV requested data from these schools. In addition, primary and secondary contacts were called to locate youth during Fall 1987 to set up interviews. Although the survey-related tracking information was not available in time to be included as part of this year's school data tracking, it will be useful next year.

Appendix B) are also examined in order to determine the robustness of the pooled all-site findings.

Both the pooled and site-by-site analyses show that the first summer of STEP did not substantially affect any of the five school-year behavior or performance variables during the mid-program year. Although STEP had little impact after the first summer for the sample as a whole, the first summer was effective for particular subgroups of youth, and differential impacts by gender, race/ethnicity, site and school-year support exposure were explored. The most significant findings were that Hispanic treatment youth had better attendance than controls, while increased contact with SYS advocates could be correlated with credits earned and promotion rates.

Attendance

The number of days in the school year varies slightly among the school districts within STEP demonstration cities. These differences were standardized by dividing days absent for each student by the total number of days in the school year to derive the percentage of the school year that the student missed.

Control youth figures indicate that Cohort II youth were absent an average of 11.44 percent of the school year. Table VII.1 shows that for the treatment sample as a whole, STEP had no effect on absenteeism.[68] The estimated impact of 0.45 percentage points is both numerically small and statistically insignificant.

The impact of STEP on absenteeism also did not vary significantly by gender, race/ethnicity or site. STEP appeared to be most effective at decreasing absenteeism among Hispanics, for whom absences were reduced by 15.7 percent, or 1.8 percentage points. Statistically, this impact does not differ from the other racial/ethnic subgroup impacts; however, this result matches that seen in the Cohort I analysis, where Hispanics showed a 14 percent reduction in absenteeism. This common estimate of STEP's impact for both cohorts provides strong evidence that STEP was more effective in reducing absenteeism among Hispanics than other racial/ethnic groups.

Among Cohort II youth, STEP's impact on absenteeism did not differ statistically across sites. Numerically, however, STEP's largest impact was in Boston (-1.40 percentage points or a 12.2% reduction in absenteeism). That STEP was most effective in reducing absenteeism in Boston is supported by the results

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[68] This analysis does not include sample members from San Diego, where attendance is not kept on a student's historical file.

Table VII.1

THE ESTIMATED IMPACTS OF STEP ON ABSENTEEISM AND TEST SCORES IN READING AND MATH DURING THE INTERVENING SCHOOL YEAR BY GENDER, RACE/ETHNICITY AND SITE FOR COHORT II

Group	Percentage of the Year Absent[a]	Reading Scores in NCEs[b]	Math Scores in NCEs[b]
Overall	0.45%	-1.98***	0.36
Gender			
Male	0.16	-2.70**	0.50
Female	0.71	-1.34	0.25
Race/Ethnicity			
Asian	0.73	-1.43	0.02
Black	0.08	-1.35	1.31
Hispanic	-1.80	-3.32**	-1.27
White & Other	3.37*	-3.01	0.31
Sites			
Boston	-1.40	-2.18	1.41
Fresno	0.20	-3.25**	0.16
Portland	1.86	n.a.	n.a.
San Diego	n.a.	1.53	0.57
Seattle	1.28	-0.81	-0.91
School-Year Support Exposure	##		
No contacts	-7.01	-3.06	-1.81
Contacted in 4 months or fewer	3.39**	-0.77	-0.67
Contacted in 5 months or more	-0.09	-0.82	1.60*
Sample Size	959	764	780

n.a. = not available.

Note: Of the 1,635 randomized members of Cohort II, 20 to 25 percent had missing data on at least one variable in the regression. Control group means are: percentage of year absent--11.44 percent, reading score--44.33, math score--52.45. None of the subgroup impacts differed from each other at even a 0.10 level.

[a] San Diego did not provide absenteeism data.

[b] Portland test scores were omitted because they were not comparable to those in other sites. Only 55% of the San Diego youth had test data.

***Indicates that the impact is statistically different from zero at the 0.01 level.

**Indicates that the impact is statistically different from zero at the 0.05 level.

*Indicates that the impact is statistically different from zero at the 0.10 level.

##Indicates that the impact differs with respect to this characteristic at a 0.05 level of significance.

observed for Boston's Cohort I youth, whose absenteeism was reduced by 20 percent.

The rate of absenteeism did differ among individuals who received different amounts of school-year support. Three groups were examined: individuals who were never contacted by a counselor/advocate during the school year, those who received one or more contacts in four school months or less, and those who received one or more contacts in five or more school months. Table VII.1 shows that individuals with less than five contacts were much more likely to be absent from school than both those who were never contacted and those who were contacted most often. The differential effect is likely due to inherent differences in the types of youth who received different amounts of contact.

Test Scores

The third and fourth columns of Table VII.1 show that the program impact on reading and math scores seen at the end of the first summer of STEP (as reported in Sipe, et al., 1987) did not persist through the first school year.[69] Treatment youth scored significantly lower on standardized reading test scores (measured in NCEs that range from 0 to 100) than did controls, while there was no effect on math scores. STEP participants scored 1.98 NCEs lower in reading and 0.36 NCEs higher in math than controls. In the absence of the program, the average scores would have been 44.33 in reading and 52.45 in math. Table VII.1 also does not provide any evidence that STEP's impact varied by gender, race/ethnicity, site or SYS exposure. The results from the aggregate sample (that combines information on the intervening school year for both Cohorts I and II, shown in Appendix Table B.18) similarly indicate that STEP had little effect on standardized test scores after the first summer.

A site-by-site analysis of scaled scores provides alternative estimates of the impact of STEP on reading and math, and are presented in Table VII.2. As discussed earlier, scaled scores from different standardized tests are not comparable and cannot be pooled across sites. To compare the estimated impacts in this table across sites, the estimated impact is divided by the mean scaled score of the control group for each site. Thus, for example, treatments in Boston scored 0.67 percentage points higher in math than controls, whereas in Fresno they scored only 0.13 percentage points higher. The site-by-site analysis presents a very similar story to the all-site analysis--reading impacts were small and negative, while the math impacts were small and positive.

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[69] Because Portland's test data were not comparable to those of the other sites, Portland is excluded from this analysis.

Table VII.2

THE ESTIMATED IMPACTS OF STEP ON SCALED TEST SCORES DURING THE INTERVENING SCHOOL YEAR BY SITE FOR COHORT II

	Scaled Reading Scores		Scaled Math Scores	
Site (Standarized) Test Use)	Estimated Impact as a Percentage of the Control Group Mean	Sample Size	Estimated Impact as a Percentage of the Control Group Mean	Sample Size
Boston (MAT)	-.87%	209	.67%	214
Fresno (CAT)	-.81**	186	.13	186
Portland[a] (PALT)	.64*	223[a]	.84**	223[a]
San Diego[b] (CTBS)	-.55	185[b]	1.18	195[b]
(MAT)	-.18		2.76**	
Seattle (CAT)	-.85	176	-.87	177
(MAT)	.83		.47	

[a] In Portland, only individuals who had not already reached a certain minimum level of competency were given a test. Thus, the sample in Portland is not comparable to the samples in other sites.

[b] In San Diego, the school district administered the CTBS only to 7th-, 9th- and 11th-graders. Thus, only 55 percent of our sample took the CTBS. Several weeks later, returning treatment and controls were given the MAT before participation in the second summer of STEP. We estimated the impact of STEP using both sets of data.

**Indicates that the impact is statistically different from zero at the 0.05 level.

*Indicates that the impact is statistically different from zero at the 0.10 level.

Because only 55 percent of the Cohort II youth in San Diego were given standardized tests, the robustness of these results were tested by substituting the scores that returning treatments and controls in San Diego achieved on their 1987 STEP-administered pretest scores (MAT) for the school-administered test scores (CTBS). By doing so, we obtained data on 80 percent of San Diego Cohort II youth. The estimated impact on math scores using this sample (shown in Table VII.2) was greater and was statistically significant.[70] The results for reading scores, however, were not significant.[71]

Portland's test data were not comparable to data obtained at other sites because Portland administered its tests only to those students who had not reached a minimum level of competency in reading and math. Thus, Portland test data were not pooled with the other data in the analysis of overall test scores. The estimated effect of STEP on Portland youth, as shown in Table VII.2, was positive but small for both math and reading test scores. Treatment youth who had not reached the minimum level of competency were slightly more likely to do so after their first summer of STEP participation than were control youth.

Overall, it appears that the first summer of STEP did not result in Cohort II treatment youth scoring substantially higher on standardized reading or math tests during the mid-program school year than they would have otherwise.

Credit Accumulation

The total number of credits needed to graduate varies considerably among school districts. To compare academic progression across sites, the reported number of credits are standardized to a common metric by dividing the number of credits earned by the total number of credits needed to graduate.[72] As mentioned earlier, because eighth-graders do not earn high school credits, our analysis is restricted to students who were in the ninth grade or above during the 1986-87 school year.

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[70] To get some idea of whether the test scores of returnees differed systematically from non-returnees, we compared their school-administered (CTBS) test scores. We found no statistical difference between the two groups.

[71] We also analyzed impacts on second-summer pretest for returning youth in Seattle. Although, like the in-school test results, the impacts were not significant, unlike the in-school analysis, treatment youth scored higher on the MAT than control youth.

Table VII.3 indicates that STEP participation had no effect on credit accumulation for the sample as a whole. Treatments and controls earned about the same percentage of credits, and treatment youth who were in the ninth grade or higher earned only 1.5 percent (0.26 percentage points) more credits toward graduation than did control youth. There also was no significant variation in the impact by gender, race/ethnicity or site. The site-by-site estimates also show small, insignificant results. Credit accumulation did vary, however, by the level of school-year support youth received. Youth who were successfully contacted in five or more school months earned more credits than control youth and treatment youth who were contacted less frequently. Youth who could not be contacted at all, on the other hand, earned far fewer credits than the average control youth. This variation in credit accumulation could indicate that SYS participation helped youth with their course work. However, youth who were easier to contact may have been those who would have done better even without school-year support.

Probability of Promotion

Only two sites, Boston and Seattle, provided end-of-year promotion information for school year 1986-87. In those two sites, STEP did not appear to significantly increase the chance that Cohort II treatment youth would pass to the next grade. Ninety percent of the control youth in Boston and Seattle were promoted at the end of the school year, compared with 89 percent of the treatment youth.

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[72] In addition to the metric by which credits were reported, the method by which sites reported credits also varied. Fresno, Portland and Seattle reported annual credits, whereas the other two sites reported cumulative credits. However, because the number of cumulative credits at the beginning of the 1986-87 school year were the same for treatments and controls, the difference in their cumulative credits at the end of the year is an estimate of STEP's impact, just as the difference in annual credits between treatments and controls is an estimate of STEP's impact. To estimate this impact based on a pooled sample, the following specification was estimated:

$$\text{cumulative credits} = a + bX + cT$$
$$\text{annual credits} = d + eX + cT$$

where X is a vector of personal characteristics, and a through e are regression coefficients to be estimated. The estimate on the treatment variable, T, is constrained to be the same in the two equations.

Table VII.3

THE ESTIMATED IMPACTS OF STEP ON CREDITS EARNED AND GRADE PROMOTION DURING THE INTERVENING SCHOOL YEAR BY GENDER, RACE/ETHNICITY AND SITE FOR COHORT II

Characteristic	Percentage of Graduation Credits Earned	Probability of Promotion[a]
Overall	0.26%	-0.94%
Gender		
Male	-0.33	-4.27
Female	0.49	2.01
Race/Ethnicity		
Asian	-0.06	-3.35
Black	1.23	0.43
Hispanic	-0.25	0.82
White & Other	-2.53	-4.70
Sites		
Boston	1.76	-2.46
Fresno	0.85	n/a
Portland	-2.06	n/a
San Diego	0.07	n/a
Seattle	0.96	0.72
School-Year Support Exposure	###	
No Contacts	-17.04***	-10.16
Contacted in 4 months or fewer	-6.64***	-6.23
Contacted in 5 months or more	1.80***	2.14
Sample Size	1,044	488

Note: Controls earned, on average, 17.49 percent of the needed total of graduation credits and 90.3 percent were promoted.

[a] The probability of promotion impacts were estimated using logit analysis. Only Boston and Seattle are included in the analysis because they were the only two sites that reported promotion at the end of school-year 1986-87.

***Indicates that the impact is statistically different from zero at the 0.01 level.

###Indicates that the impact differs with respect to this characteristic at a 0.01 level of significance.

This finding contradicts the estimated impact seen for Cohort I treatment youth. The Cohort I analysis used data from all five sites and showed that treatments were more likely to be promoted than control youth. The average Cohort I STEP participant was 22.1 percent (5.27 percentage points) less likely to fail than his or her control counterpart. However, only 10 percent of Cohort II controls in Boston and Seattle failed, while 24 percent of all Cohort I controls failed. The higher failure rate among Cohort I youth increased the likelihood that an effect of STEP could be detected. The differential effect for Cohorts I and II may also reflect the difference in available data, since the analysis of promotion among Cohort I youth included data from all five sites, while the Cohort II analysis was limited to Boston and Seattle.

Although there is no statistically significant variation among subgroups (probably due to the small sample size), the variation in the impact by SYS exposure mimicked the findings for credit accumulation; youth with above average SYS exposure were the most likely to be promoted, while those with no SYS mentoring were least likely to be promoted. The similarity of these two related impacts gives us confidence that the pattern is not a statistical fluke.

Probability of Dropping Out

As discussed earlier, it is very difficult to determine who is a dropout. We considered two definitions: a strict definition that included only students who were recorded as dropouts by the school district and a less restrictive definition that included all recorded dropouts plus individuals who had withdrawn for an unstated reason. Some of the second definition's additional inclusions may be individuals with whom the schools and P/PV have simply lost touch; however, many are probably true dropouts.

Unfortunately, the difference in what these two definitions measure is unclear, since much of the variation among sites is caused purely by the way they record withdrawals. Portland, for example, records almost all not-within-district transfers as withdrawals due to "other reasons." Thus, almost no one is explicitly a dropout. In San Diego, on the other hand, unless the school knows that a student has transferred or was specifically exempted from the truancy laws, the youth is recorded as "dropped (unknown)." Thus, dropout rates among controls varied considerably by site and definition. According to the strict definition, the dropout rates were 3.6 percent in Boston, 4.3 percent in Fresno, 0.0 percent in Portland, 21.9 percent in San Diego and 2.4 percent in Seattle. According to the less restrictive definition, the rates were 9.1 percent in Boston, 16.6 percent in Fresno, 17.3 percent in Portland, 30.6 percent in San Diego and 12.1 percent in Seattle.

When the dropout rates of treatments and controls are compared, no significant differences are found using either definition. According to the restricted measure, 6.38 percent of controls across all sites were dropouts. Using this definition, treatment youth were 1.34 percentage points less likely to be dropouts (significant at the 0.10 level). Using the less restrictive measure, 17.06 percent of the controls were dropouts, compared with 28.53 percent of the treatments. Thus, it is unclear whether STEP enrollment affected the probability that an individual would drop out of school after the first summer. Because we admittedly have no good method of measuring dropout rates, questions about school retention may be better addressed by examining grade promotion and graduation, both of which are well-defined, or analyzing the self-reported dropout status data that will be available from the follow-up survey.

Summary: School Data Outcomes

Overall, no school outcome was greatly affected by the first summer and SYS component of STEP. Reading test scores of treatments were somewhat lower than those of controls, but the magnitude of the difference is trivial. The average NCE score for the treatments was 42.35, compared with 44.33 for controls.

These results are similar to the 1986 results with one major exception. Cohort I treatments were 22.1 percent less likely to fail than controls, while Cohort II treatments, with data limited to youth in Boston and Seattle, were equally likely to fail. The Cohort I impact on promotion had been considered encouraging, though it was viewed cautiously because a corresponding increase in the number of credits earned was not found. The results for Cohort II raise further questions regarding the generalizability of the estimated Cohort I promotion impact. However, the lack of program impact on promotion rate may be due to much higher rates of promotion among Cohort II controls--a factor that left the program with less potential to affect promotion. Furthermore, all differences may be due primarily to the availability of data in only two sites for Cohort II.

School-year support information collected on Cohort II youth permitted analysis of the differential effect of STEP on youth who received various levels of SYS exposure. Youth who received some SYS services earned more credits and were more likely to be promoted than youth who received no services. These findings are consistent with the hypothesis that SYS helps youth get through school. However, it is also consistent with the hypothesis that youth who are easily contacted would have done better even in the absence of SYS services. Empirically, the two hypotheses cannot be distinguished.

CHARACTERISTICS OF COHORT II RETURNEES

All Cohort II treatment youth were invited to participate in a second summer of work experience, remediation and life skills instruction in 1987. As discussed in Chapter III, P/PV decided not to retest control youth who were not being offered second summer jobs. Only San Diego and Seattle offered control youth a second-summer job. Thus, second-summer test and questionnaire data were collected for control youth only in these two sites.

The returning sample looks somewhat different from the original Cohort II treatment group. As discussed in Chapter V, treatment youth who returned were more likely to be 15 years old than 14 years old when they entered the program and were more likely to have had employment experience prior to STEP than non-returning youth. Returning youth also had higher average reading and math scores, and better school attendance, and were less likely to have a history of grade retention than non-returnees. Finally, significantly fewer returning students than non-returnees were sexually experienced when they entered STEP.

Returning control youth closely resembled the original control group in Seattle. However, some differences between returning and non-returning control youth were noted in San Diego. Returning control youth in that site had higher average test scores, less previous work experience and less sexual experience than non-returning controls. (For a more complete discussion of the Cohort II returnees in relation to the original Cohort II sample, see Chapter V.)

Table VII.4 presents selected information describing the Cohort II treatment returnees at the beginning of their second summer in STEP. As 1987 MAT pretest scores indicate, Cohort II treatment youth were performing at about the 6.6 grade level in reading and the 7.8 grade level in math. Since close to half of these youth were enrolled in the 10th grade or higher during the 1986-87 school year, they are still performing well below their grade of enrollment.

Some notable changes in the characteristics of the Cohort II returnees occurred during the year subsequent to their first summer of STEP. When they began STEP, 71 percent of Cohort II treatment returnees reported that they expected to obtain some post-high school education. By the beginning of the second summer, the percentage expecting post-high school education had increased to 78 percent. Problems with school attendance, however, appear to have increased during the intervening year. While 22 percent of these youth reported more than 10 absences during the 1985-86 school year, 29 percent reported more than

Table VII.4

SELECTED CHARACTERISTICS OF RETURNING COHORT II TREATMENT YOUTH AT BEGINNING OF SECOND SUMMER, BY SITE

	All Treatments	Boston	Fresno	Portland	San Diego	Seattle
Mean MAT Reading Score	731.60	728.62	702.70	756.58	734.97	734.95
Grade Equivalent	6.6	6.5	5.1	8.1	6.8	6.8
Mean MAT Math Score	732.91	707.87	711.47	738.07	783.45	720.39
Grade Equivalent	7.8	7.0	7.1	8.0	10.4	7.4
Grade During SY 1986-87						
9th or Below	57.4%	95.7%	46.6%	52.3%	51.7%	49.5%
10th or Above	42.6	4.3	53.4	47.6	48.2	50.5
Percentage Absent More Than 10 Days During SY 1986-87	28.8%	23.9%	35.6%	34.1%	18.4%	30.9%
Percentage Expecting Post-High School Education	78.1%	70.0%	81.8%	80.2%	79.3%	77.7%
Knowledge of Contraception	7.57	6.59	7.69	7.93	8.70	6.81
Knowledge of Availability of Contraception	5.06	4.28	5.15	5.21	5.84	4.71
Percentage With Sexual Experience	48.7%	47.1%	46.4%	57.3%	40.7%	51.6%
Percentage Ever Pregnant	4.7%	4.9%	6.5%	4.4%	-	7.4%
Percentage Ever Fathered a Child	2.6%	6.7%	2.4%	4.9%	-	-
Sample Size	426	71	88	86	87	94

10 absences during 1986-87.[73] Finally, the percentage of treatment youth with sexual experience increased by about nine percentage points (from 40% to 49%) during the year between the beginning of STEP and the beginning of the second summer.

In San Diego and Seattle, returning treatment and control youth (see Table VII.5) began the second summer of STEP at about the same reading level, but treatment youth had average math scores equivalent to approximately 0.8 of a grade level higher than control youth.[74] This change occurred despite the fact that for the demonstration as a whole, no differences in Spring 1987 test scores were found. Returning treatment youth in San Diego and Seattle reported worse attendance than controls during SY 1986-87. Twenty-five percent of treatments and only 19 percent of controls reported missing more than 10 days of school. In all other characteristics, treatment and control returnees in these two sites were very similar going into their second summer of STEP.

DESCRIPTION OF SECOND-SUMMER AND 15-MONTH OUTCOMES

All Cohort II youth who returned for a second summer of STEP took the MAT Intermediate Level reading and math tests and completed questionnaires at the beginning and end of the second summer. These data provide the primary indicators of second-summer outcomes for returning STEP participants. In addition to the second-summer outcomes, outcomes over the 15-month period are examined.

These analyses are limited by the lack of second-summer data on the Cohort II control group. Primarily, descriptions of the changes in outcome measures over the second summer and the 15-month period are presented. Changes over the second summer are calculated as the difference between the 1987 pretest and posttest outcome levels. Changes over the 15-month period are calculated as the difference between the 1986 pretest and 1987

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[73] The school data indicate that 50 percent of the returnees were absent more than 10 days in the 1986-87 school year. Thus, it appears that self-reported absenteeism is severely underreported.

[74] The difference in San Diego was particularly large--treatment youth scored almost two grade levels above control youth in math at the beginning of Summer 1987. This may partially reflect the fact that a higher percentage of treatments than controls were enrolled in the 10th grade during SY 1986-87. The difference may also reflect the greater tendency of Asian treatment youth to return than Asian control youth. However, the site-by-site school data analysis of test scores reported in Table VII.2, though not statistically significant, indicated that San Diego treatments outperformed controls on the CTBS, controlling for race/ethnicity and grade.

Table VII.5

SELECTED CHARACTERISTICS OF RETURNING COHORT II YOUTH IN SEATTLE AND SAN DIEGO

			San Diego		Seattle	
	All Treatments[a]	All Controls[a]	Treatments	Controls	Treatments	Controls
Mean MAT Reading Score	734.96	736.93	734.97	737.33	734.95	736.58
Grade Equivalent	6.8	6.9	6.8	6.9	6.8	6.9
Mean MAT Math Score	750.70	730.05	783.45	751.28	720.39	711.66
Grade Equivalent	8.5	7.7	10.4	8.5	7.4	7.1
Grade During SY 1986-87						
9th or Below	50.6%	51.2%	51.7%	60.3%	49.5%	43.3%
10th or Above	49.4	48.8	48.2	39.8	50.5	56.6
Percentage Absent More Than 10 Days During SY 1986-87	24.9%	19.0%	18.4%	17.9%	30.9%	20.0%
Percentage Expecting Post-High School Education	78.5%	81.0%	79.3%	79.5%	77.7%	82.2%
Knowledge of Contraception	7.72	5.82	8.70	5.73	6.81	5.91
Knowledge of Availability of Contraception	5.25	4.00	5.84	4.29	4.71	3.75
Percentage With Sexual Experience	46.3%	47.2%	40.7%	44.0%	51.6%	50.0%
Percentage Ever Pregnant	4.0%	2.5%	-	2.4%	7.4%	2.6%
Percentage Ever Fathered a Child	-	5.9%	-	5.6%	-	4.1%
Sample Size	181	168	87	78	94	87

[a] These numbers refer to all youth who returned for a second summer in San Diego and Seattle.

posttest levels. We tested to determine whether the estimated changes are equal to zero. This approach is used to examine both learning- and life skills-related outcomes.[75]

The primary exception to this strategy is the analysis of San Diego and Seattle data. Because the San Diego and Seattle sites offered SYETP jobs to Cohort II control youth during the 1987 summer, these sites were able to collect questionnaire and test data for returning control youth. Thus, we are able to estimate net impacts of program participation on the learning outcome measures.[76] Although we cannot generalize these results to the demonstration as a whole, they provide some evidence of the effectiveness of the program.

In addition to the summer questionnaire data, information was collected from Cohort II treatment and control youth from all five sites in the first wave of follow-up interviews. These interviews were conducted between October 1987 and January 1988--from one and a half to five months after Cohort II youth completed the 15-month STEP program. These data serve as proxies for end-of-program life skills-related outcomes[77] and allow us to estimate short-term impacts of the complete 15-month program on contraceptive knowledge, sexual initiation and contraceptive use across sites.

Description of Learning Outcomes

Mean MAT reading and math scores at baseline and changes in these mean scores for each of the three subsequent test administrations are presented in Table VII.6 for the overall group of returning treatment youth and for returning San Diego and Seattle treatment and control youth. Although the returning youth are a subset of the original Cohort II treatment sample,[78] the first-summer changes for the returnees mirror those experienced for the ori-

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[75] For the learning analysis, a multivariate analysis using two-stage least squares techniques provided estimates of the average impact of an additional hour of treatment (i.e., remediation and life skills classes) on posttest reading and math scores.

[76] Multivariate regression models that parallel those estimated for first-summer impacts were estimated for San Diego and Seattle returnees.

[77] Multivariate regression and logit models on birth control knowledge, sexual activity and contraceptive use are estimated.

[78] Returnees have a few significant differences from the original treatment group, but are generally similar.

Table VII.6

CHANGES IN MEAN READING AND MATH SCORES OVER 15 MONTHS FOR RETURNING COHORT II TREATMENTS AND SAN DIEGO AND SEATTLE CONTROLS

	Returning	San Diego and Seattle		
	Treatments	Treatments	Controls	Difference
Reading Scores				
1986 Pretest	724.05	729.08	726.88	2.20
First-Summer Change	-6.42***	-5.80	-20.46***	14.66***
School-Year Change	13.97***	11.68***	30.51***	-18.83***
Second-Summer Change	-0.72	1.85	-20.35***	22.20***
15-Month Change	6.83***	7.73**	-10.30**	18.03***
Math Scores				
1986 Pretest	705.87	719.65	707.76	11.89
First-Summer Change	8.24***	12.25***	-11.45*	23.70***
School-Year Change	18.80***	18.81***	33.75***	-14.94*
Second-Summer Change	-1.66	-8.61*	-18.29***	9.68
15-Month Change	25.38***	22.44***	4.01	18.43**
Sample Size	426	181	168	349

***Indicates that the change is statistically different from zero at the 0.01 level.
**Indicates that the change is statistically different from zero at the 0.05 level.
*Indicates that the change is statistically different from zero at the 0.10 level.

ginal cohort (-6.42 vs. -6.67 in reading and 8.24 vs. 7.03 in math).

Second-Summer Outcomes

The differences in second-summer test scores indicate that treatment youth experienced no significant changes in either reading or math during the second summer. These outcomes are considerably better than those seen for Cohort I (who lost about 17 points in reading and 4 points in math) in their second summer.

The reading losses Cohort II treatment youth experienced during the first summer were eliminated in the second summer. Cohort II treatment youth did not, however, experience additional gains in math during the second summer. Without comparable data on a control group, however, we cannot determine the impact of participation in STEP during the second summer.

By examining attendance data, however, we can determine whether increased exposure to STEP resulted in better posttest scores. Ideally, we would estimate the marginal impact of each additional hour beyond the impact on youth with average attendance. However, we can only determine the average impact of each hour of classroom participation. The results of this analysis indicate that the average impacts of an hour of classroom training are 0.84 points in reading and 1.31 points in math in the second summer (both are statistically significant).[79]

Over the course of the STEP demonstration we have consistently found that treatments outperform control youth, even though treatments have sometimes shown losses. One might hypothesize therefore, that test data on the control group in the second summer, if available, would indicate losses of a greater magnitude than that observed for treatment youth.

The control group data collected in San Diego and Seattle provide an alternative way of gauging STEP's effect. As indicated in Table VII.6, control youth in San Diego and Seattle experienced greater losses during the second summer of STEP than their treatment counterparts. Treatment youth in these sites gained an insignificant 1.85 points in reading while control youth lost 20.35 points (1.1 grade levels). The differential performance in math is smaller; treatment youth lost 8.61 points (0.3 of a grade), while control youth lost 18.29 points (0.6 of a grade). The estimated net impact of treatment for San Diego and Seattle youth during the second summer also indicates the benefit of participating in a second summer of STEP. Regression analysis, which controlled for skill levels measured at the start of the second summer (i.e., 1987 pretest scores), indicates that treatment youth outperformed control youth by 14.09 points in reading and 21.74 points in math during the second summer. The positive impact of STEP in these sites during the second summer is equivalent to 0.7 of a grade in reading and 0.8 of a grade in math, both of which are statistically significant. These impacts are larger than those experienced by San Diego youth during their first summer of STEP. Conversely, these second-summer impacts are smaller than those experienced by youth in Seattle during their first summer.

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[79] Two-stage least squares techniques were used to estimate these impacts.

We have no way of knowing whether the impact of STEP in the other three sites was as strong during the second summer as it was in San Diego and Seattle. It is probable, however, that control group performance at the other three sites would have been worse than that experienced by the treatment youth had we obtained measures for this group.

Fifteen-Month Outcomes

Table VII.6 contains changes in mean reading and math scores for returning Cohort II youth from the beginning of STEP (1986 pretest) to the completion of STEP (1987 posttest). The 15-month program period includes two summers when we would expect youth to experience learning losses, and one school year during which we would anticipate learning gains.

Over 15 months of STEP, Cohort II treatment youth experienced gains in both reading and math. In reading, the gain of 13.97 during the school year was partially offset by small summer losses, resulting in a significant gain of 6.83 points (equivalent to 0.3 of a grade) over 15 months. In math, where youth had gains during the first summer as well as during the school year, Cohort II treatment youth gained 25.38 points (a full grade level) during their STEP enrollment. Over the 15-month program, the estimated average impact of each treatment hour is 0.08 points in reading (not significant) and 0.43 points in math (significant at the 0.05 level).

The results for San Diego and Seattle returnees again provide some insight into what the impact of participation over the complete 15-month program might be. As was true for the treatment youth overall, the school-year gain in reading experienced by the San Diego and Seattle treatments was not completely offset by losses during the two summers. Treatment youth in San Diego and Seattle gained about 0.4 of a grade level over their baseline reading level. Their control counterparts, however, lost 10.30 points (or 0.6 of a grade level), dropping from a 6.4 level at the start of the program to 5.8 at the end. In math, larger summer losses almost completely offset large school-year gains for the controls. In contrast, STEP produced gains during the first summer and stemmed losses during the second summer for the treatments, producing a net gain in math over the 15-month period. Since controls gained significantly more than treatments during the school year, the treatment/control differential in both reading and math for the 15-month program is largely due to differential summer learning.

The net impact of 15 months of STEP for San Diego and Seattle youth was estimated using a multiple regression model that controlled for 1986 baseline differences between the treatment and control youth. In reading, the impact of the full program was equivalent to 0.9 of a grade (statistically significant at the 0.01 level). The net impact on math was equivalent to 0.7 of grade (also significant at the 0.01 level). As with the second-summer outcomes, we cannot generalize these impacts to the remaining Cohort II treatment youth. We can only infer that the positive impacts of STEP in San Diego and Seattle suggest that participation in the full 15-month program is beneficial in terms of improving basic skills. Given the similar first summer performance of the third cohort, we would expect similar outcomes during the school year and second summer for Cohort III as well.

Return to School

Participation in STEP did not affect school enrollment in the short term. In Fall 1987, school districts provided enrollment information for the 1987-88 school year for Cohort II youth. Approximately 90 percent of all Cohort II youth (90.6% of treatments and 88.8% of controls) returned to school in Fall 1987. Among the Cohort II treatment youth who participated in a second summer of STEP, 89.2 percent returned to school in 1987. In San Diego and Seattle, 91.0 percent of treatment youth and 90.8 percent of control youth who returned for the second summer of STEP returned to school. These rates of school reenrollment for treatments and controls are not significantly different.

Description of Life Skills Outcomes

Returning Cohort II youth completed questionnaires similar to those administered in the first summer of STEP at both the beginning and end of the second summer. Measures of contraceptive knowledge and knowledge of the availability of contraceptives that were identical to those asked in the first summer provided information on the retention of knowledge gained during the first summer and helped determine whether knowledge continued to increase during the second summer. Youth were also asked to provide behavioral information similar to that requested during the first summer on sexual experience, recent sexual activity and use of contraceptives. Measures of health and substance abuse knowledge, and knowledge of appropriate work-related behavior were also included in the second-summer questionnaires. (These topics are the subject of individual lessons only in the second summer of the life skills curriculum.)

Since second-summer questionnaire data were not collected for the majority of control youth, we cannot determine program impacts for these life skills outcomes. However, Cohort II treatment and control youth in all sites were interviewed shortly after program completion (Fall 1987). Many of the same fertility-related measures included in the summer questionnaires were asked during these interviews. The short-term impact of 15 months of STEP on contraceptive knowledge, on sexual activity and on contraceptive use can be determined from these data.

Fertility-Related Knowledge

Table VII.7 shows mean scores on the measures of knowledge of contraception at baseline and changes in these mean scores at subsequent measuring points for returning Cohort II treatment and control youth in San Diego and Seattle. Mean scores for the returning treatment youth clearly show that knowledge of contraception significantly increased over the 15-month program. As was true for the entire group of Cohort II treatments (reported in Sipe, et al., 1987), returnees increased their performance on this test by approximately 50 percent during their first summer, in 1986. Most of this increase in knowledge gained during the first summer of STEP was retained by returning treatments during the intervening school year, as scores were down only about 10 percent at second-summer baseline. Over the second summer of STEP, the level of knowledge increased significantly once again.

When these youth entered STEP, they were able to correctly answer an average of five of 12 questions on knowledge of contraception. By the end of the 15 months, they correctly answered an average of almost nine questions. The pattern was very similar for returning treatments' performance on the knowledge of contraceptive availability test. Youth increased their average scores on this test from about three of eight questions at baseline to almost six questions by the end of the 15-month program.

The data from San Diego and Seattle provide an indication of the increase in treatments' knowledge relative to the control group's. The results suggest that the observed knowledge increases are likely due to STEP participation rather than simply to maturation. Although control youth gained some knowledge during the intervening school year (and thus for the overall program period), the knowledge gain shown by San Diego and Seattle treatment youth over the 15 months was significantly greater by more than four questions on the 12-item measure and more than two questions on the eight-item measure.

To further measure STEP's impact on fertility-related knowledge, a nine-item subset of the 12-item knowledge of contraception scale was included in the follow-up interview. At baseline, Cohort II youth correctly answered four of these nine items. Following completion of STEP, treatment youth were able to

Table VII.7

CHANGES IN MEAN KNOWLEDGE OF CONTRACEPTIVE METHODS AND AVAILABILITY OVER 15 MONTHS FOR RETURNING COHORT II TREATMENTS AND SAN DIEGO AND SEATTLE CONTROLS

	Returning Treatments	San Diego and Seattle		
		Treatments	Controls	Difference
Mean Knowledge of Contraceptive Methods[a]				
1986 Pretest	5.10	5.08	4.88	0.19
First-Summer Change	2.86***	3.33***	0.05	3.28***
School-Year Change	-0.38***	-0.68***	0.83***	-1.51***
Second-Summer Change	1.18***	1.24***	-0.16	1.40***
15-Month Change	3.66***	3.89***	0.72***	4.61***
Mean Knowledge of Contraceptive Availability[b]				
1986 Pretest	3.00	3.07	3.14	-0.17
First-Summer Change	2.24***	2.67***	0.11	2.56***
School-Year Change	-0.17*	-0.49***	0.72***	4.61***
Second-Summer Change	0.80***	0.95***	-0.08***	1.03***
15-Month Change	2.87***	3.14***	0.75***	2.39***
Mean Knowledge of the Consequences of Teen Pregnancy[c]				
1986 Pretest	0.89	0.86	0.98	-0.12*
First-Summer Change	0.28***	0.41***	-0.12***	0.53***
School-Year Change	0.50***	0.38***	0.66***	-0.28***
Second-Summer Change	0.03	0.06	-0.02	0.08
15-Month Change	0.81***	0.86***	0.53***	0.33***
Sample Size	422	181	167	

[a] Mean number correct responses on a 12-item scale.

[b] Mean number correct responses on an eight-item scale.

[c] Mean number correct responses on a two-item scale.

***Indicates that the change is statistically different from zero at the 0.01 level.

*Indicates that the change is statistically different from zero at the 0.10 level.

correctly answer nearly six items, while control youth increased their average score by only half a correct answer.

Regression results on 1,277 Cohort II youth who provided both baseline and follow-up interview information (78% of the randomized sample) allow us to estimate the net impact of STEP participation on knowledge of contraception. As shown in Table VII.8, STEP participation significantly increased youth's knowledge of birth control--by 1.31 questions (a factor of 28%). This significant impact on overall sample was seen for both genders, all all racial/ethnic groups and all sites. However, the degree of improvement differed significantly across racial/ethnic groups and across sites. Consistent with previous analyses of knowledge of contraception, the improvement in knowledge of birth control was large for Asian youth and San Diego youth, and was significantly smaller for Boston youth.

Clearly, participation in STEP significantly increased youth's knowledge of birth control beyond what they would have gained as part of the normal maturation process and/or as the result of any sex education received during the intervening year. In addition, treatment youth retained most of what they learned in the first summer of STEP over the subsequent year.

Reported Sexual Behavior

Self-reports on whether treatment youth ever had sex were obtained at baseline and endline in 1986 and 1987. As we would expect as these youth mature, the percentage who reported that they were sexually experienced increased from the end of the first summer to the beginning of the second summer of STEP and continued to increase during the second summer. Among returning treatment youth for whom we have measures of prior sexual experience at both baseline 1986 and endline 1987, the percentage with sexual experience increased by 39 percent (from 35.2% to 48.9%). The question that remains unanswered by available data is whether this increase is smaller than would have occurred in the absence of STEP.

The San Diego and Seattle data gives some indication of the relationship between treatment and control youth's sexual behavior during the 15 months of STEP. In these sites, the percentage of treatments reporting sexual experience increased by 10.6 percentage points over the 15 months, while the percentage of control youth doing so increased by 6.8 percentage points. Although the percentage of treatment youth with sexual experience increased by 3.8 percentage points more than the percentage of control youth in these two sites, the difference was not statistically significant. Thus, we cannot reject the hypothesis that this was a result of chance.

Table VII.8

NET IMPACT OF PARTICIPATION IN STEP ON CONTRACEPTIVE KNOWLEDGE FOR COHORT I OVERALL AND BY GENDER, RACE/ETHNICITY AND SITE

	Cohort II
Group	Contraceptive Knowledge[a]
Overall	1.31***
Gender	
Male	1.34***
Female	1.27***
Race/Ethnicity	###
Asian	2.02***
Black	1.16***
Hispanic	1.03***
White & Other	1.25***
Site	###
Boston	0.63***
Fresno	1.14***
Portland	1.17***
San Diego	2.51***
Seattle	1.05***

Note: The sample size for this analysis is 1,277. Impacts for each subgroup are computed as the sum of the treatment coefficient and each subgroup treatment interaction term coefficient. Significance is then determined by the appropriate F-test of the hypothesis that the sum equals zero.

[a] Mean number of correct responses on a nine-item scale.

***Indicates that the impact differs statistically from zero at the 0.01 level.

###Indicates that the impact differs with respect to this characteristic at a 0.01 level of significance.

To determine whether STEP had any overall impact on reported delay of sexual initiation, we examined the follow-up interview data shown in Table VII.9. Looking first at the change in percentage of sexually experienced control youth from baseline to follow-up, we see that in the absence of STEP, reported sexual activity increased by 6.6 percentage points. The increase in sexual activity was slightly but not significantly less for treatment youth.

Net impacts of participation on reported delay of sexual initiation were estimated using logit analysis. The results of this analysis indicate that treatment youth were 9.5 percent less likely to have had a sexual experience by the time of the follow-up interview. This difference, however, is not statistically significant. Analysis of gender subgroups provides similar results. Although both male and female treatment youth were less likely than controls to report being sexually experienced at follow-up, neither difference proved statistically significant.

A second measure of fertility-related behavior is the reported use of contraception during a youth's most recent intercourse. Among sexually experienced treatment youth who returned for a second summer, the percentage who reported using some form of birth control increased from the baseline 1986 to the end of the second summer by 9 percent (from 40.6% to 46.6%). The number of sexually experienced youth in San Diego and Seattle who responded to contraceptive use questions was so small (55 treatments and 60 controls), however, that data cannot provide even the rough comparison between treatments and controls that was possible for other outcome measures. Instead, we must rely solely on the information provided in the follow-up interviews.

At follow-up, youth reported whether they had used contraception during their most recent intercourse. As shown in Table VII.9, the percentage of sexually experienced youth reporting contraceptive use increased from 37.6 percent at baseline to 63.1 percent at follow-up among treatments, and from 38.8 percent to 59.4 percent among controls. The rate of increase among treatment youth, however, is not significantly greater than the increase among controls.

Results of the multivariate analysis indicate that treatment youth were 12.7 percent more likely to report having used contraception during their most recent intercourse, but the estimate is not statistically significant. Male treatment youth, however, were 16.9 percent more likely than controls to report contraceptive use (significant at the 0.10 level).[80]

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[80] This result from the multivariate analysis indicates an opposite effect from the apparently greater increase of contraceptive use among controls indicated by changes in percentages shown in Table VII.9.

Table VII.9

BASELINE TO FOLLOW-UP CHANGE IN THE PERCENTAGE OF COHORT II YOUTH WITH SEXUAL EXPERIENCE AND USING CONTRACEPTIVES AT MOST RECENT INTERCOURSE (BY GENDER)

	Sexual Experience[a]		Contraceptive Use[b]	
	Baseline	Change	Baseline	Change
Overall				
Treatment	38.4%	4.7***	37.6%	25.5***
Control	41.8	6.6***	38.8	20.6***
Difference		-1.9		4.9
Males				
Treatment	55.2%	0.7	42.3%	24.0***
Control	57.6%	3.2	33.6	29.4***
Difference		-2.5		-5.4
Females				
Treatment	24.5%	8.1***	26.7%	28.9***
Control	26.3%	10.0***	53.7	-0.5
Difference		-1.9		28.4**

[a] Percentage of youth reporting that they have ever had sex (n = 653 treatments, 633 controls).

[b] Percentage of sexually experienced (at baseline) youth reporting use of contraception the last time they had sex (n = 149 treatments, 160 controls).

***Indicates that the change is statistically different from zero at the 0.01 level.

**Indicates that the change is statistically different from zero at the 0.05 level.

Finally, the follow-up interview data indicate that participation in STEP has had no short-term impact on preventing pregnancies. Among treatments who were interviewed, 7.6 percent reported having been pregnant or having caused a pregnancy, compared with 8.5 percent of control youth.

Health and Substance Abuse Outcomes

Two topics included as individual lessons in the second-summer LSO curriculum that were not part of the first-summer curriculum were general health and substance abuse. While Cohort II treatments' knowledge concerning drug and alcohol abuse did not change over the summer, their knowledge of general health issues did.[81]

To determine whether knowledge about health increased during the summer, youth were asked to respond to four true/false statements on pretest and posttest questionnaires. As shown in Table VII.10, treatment youth correctly answered an average of 2.62 of these questions at the beginning of the summer. The level of correct responses increased slightly over the summer among returning treatment youth in San Diego and Seattle. Both the treatment increase of 0.21 and the control increase of 0.11 on this scale were significant, but the treatment/control difference was not. Thus, participation in STEP did not affect knowledge of general health issues in San Diego and Seattle.

Knowledge of substance abuse was measured using eight true/false questions. Returning treatment youth correctly answered an average of 4.33 of the eight questions at baseline, and their knowledge did not increase over the summer. Treatment youth in San Diego and Seattle also failed to increase their knowledge over the summer, but control youth in these sites forgot some of their knowledge of substance abuse, resulting in a significant treatment/control difference in level of knowledge at the end of the summer.

Knowledge of Appropriate Work-Related Behavior

A major focus of the second-summer LSO curriculum is work-related information and responsible behavior on the job. An eight-item scale was included in the second summer questionnaire to measure any changes in work-related knowledge as a result of STEP participation. However, returning treatment youth were able to correctly answer six of the eight items at baseline, as shown in Table VII.10, leaving little room to detect a second-summer increase. In spite of this, treatments correctly answered an

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[81] Among Cohort I treatment youth, we observed a significant increase in knowledge of substance abuse, but no change in knowledge of general health issues.

Table VII.10

BASELINE TO ENDLINE CHANGES IN MEAN HEALTH, DRUG ABUSE AND APPROPRIATE WORK BEHAVIOR KNOWLEDGE FOR COHORT II

	Returning Treatments	San Diego and Seattle		
		Treatments	Controls	Difference
Mean Health Knowledge[a]				
Pretest	2.62	2.59	2.60	-0.01
Posttest	2.72	2.80	2.71	0.09
Change	0.10**	0.21**	0.11*	0.10
Mean Drug Abuse Knowledge[b]				
Pretest	4.33	4.17	4.26	-0.09
Posttest	4.27	4.29	3.63	0.66***
Change	-0.06	0.12	-0.63***	0.75***
Work Behavior Knowledge[b]				
Pretest	6.09	6.10	6.02	0.08
Posttest	6.40	6.38	6.03	0.35**
Change	0.31***	0.28***	0.01	0.27**

[a] Mean number of correct responses on a 4-item battery.
[b] Mean number of correct responses on an 8-item battery.

***Indicates that the change is statistically different from zero at the 0.01 level.
**Indicates that the change is statistically different from zero at the 0.05 level.
*Indicates that the change is statistically different from zero at the 0.10 level.

average of half a question more at the end of the summer than at baseline, a statistically significant increase. Since control youth in San Diego and Seattle demonstrated no such increase, these data suggest that this increase in the level of work-related knowledge is significantly greater than would be expected in the absence of STEP.

Summary: Second-Summer and 15-month Outcomes

Second-summer changes in reading and math for Cohort II returnees were negligible. Over the full 15-month program period, however, returning treatment youth experienced a small but significant gain in reading (equivalent to 0.3 of a grade) and a substantial gain in math (equivalent to a full grade level).

In San Diego and Seattle, where we have second-summer test data for control youth, participation in 15 months of STEP significantly improved both reading and math scores. The net impact of participation was equivalent to 0.9 of a grade level in reading and 0.7 of a grade level in math for youth in these two sites.

Treatment youth retained much of the knowledge of contraception they gained during the first summer and this knowledge continued to increase among returning treatments during the second summer. Participation in STEP, however, has had no short-term impact on reported sexual behavior, though there has been a significant increase in reported use of contraception among males. Treatment males were 16.9 percent more likely to report haviing used contraception during their most recent intercourse than control males.

Finally, treatment youth who participated in the second summer of STEP demonstrated no increase in their knowledge of either general health issues or substance abuse. Their knowledge of appropriate work-related behavior, which was already quite high at the beginning of the summer, had increased slightly by the conclusion of STEP.

VIII. POSTPROGRAM IMPACTS FOR COHORT I

This chapter presents the first analysis of longer-term (postprogram) impacts of STEP participation for Cohort I youth, who completed participation in the STEP program in August 1986. Outcomes on measures of education, fertility and early employment are examined. These measures are obtained from two sources: school records data for SY 1986-87 (the year following Cohort I's completion of STEP) and data from follow-up interviews, conducted between October 1987 and January 1988, slightly more than a year after youth completed the 15-month program. The majority of Cohort I youth were in the 10th or 11th grade during SY 1986-87 and were in the 11th or 12th grade at the follow-up.

As discussed in Chapter II of this report, a number of changes to the STEP model were introduced following the first year of operations. These modifications, which included the new remediation curriculum (Practical Academics) and a more extensive SYS component, made the intervention experienced by Cohorts II and III stronger than that experienced by Cohort I. From the analyses discussed in previous reports and in Chapter VI of this report, we know that in-program impacts were stronger for Cohorts II and III than those observed for Cohort I. Thus, we also expect that postprogram impacts for the second and third cohorts will be stronger than the impacts detected for Cohort I. Similarly, we expect that postprogram impacts will be strongest among Cohort I youth for whom in-program results were strong.

Analyses of education-related outcomes for Cohort I indicate that treatment youth had higher math scores on standardized tests taken in Spring 1987, but show no impact on reading scores. Attendance and suspension rates were very similar for treatments and controls. Both groups had earned a similar number of credits and had reached approximately the same level of schooling. In Boston and Seattle (the only two sites where promotion data are available), treatment youth were more likely to have been promoted at the conclusion of the school year. Overall dropout rates were lower for treatment youth than for controls, but the difference was not statistically significant. Females and Hispanic youth who participated in STEP, however, had significantly lower dropout rates than their control counterparts. Finally, STEP participation had no overall impact on educational expectations.

Fertility-related outcomes show that STEP participation significantly improved contraceptive knowledge. Not only did treatment youth retain much of the knowledge they gained from STEP, they significantly outscored control youth on this measure at follow-up. Participation in STEP, however, did not affect rates of sexual initiation, recent sexual activity, contraceptive use or the incidence of pregnancy.

Employment data for Cohort I youth during the quarter encompassing July through September 1987 show that treatment and control youth were equally likely to have been working and earned similar average wages for the quarter. All these outcomes, including differences across gender, race/ethnicity and site subgroups, are discussed in greater detail in the following sections.

EDUCATIONAL OUTCOMES

In this section, we discuss the impact of STEP participation on Cohort I's attendance, standardized test performance, grade progression, graduation, rate of dropping out of school and educational expectations. The strengths and weaknesses of many of these outcome measures were discussed in Chapter VII.

Attendance and Suspension

Two measures of in-school behavior were examined--attendance and school suspensions. Attendance was obtained from school records, while suspensions were self-reported in the follow-up interview.

To calculate attendance rates across the STEP sites, we analyzed the percentage of the school year that a youth was absent rather than the number of days absent. On average, Cohort I control youth were absent 15 percent of the 1986-87 school year. Table VIII.1 shows that STEP participation had no effect on absenteeism during SY 1986-87 for the overall sample of Cohort I youth. The results were not statistically different for any gender, racial/ethnic or site subgroup.

There was also little difference in suspensions experienced by Cohort I treatments and controls during the previous two school years. Among treatment youth, 25.48 percent reported that they had been suspended at least once; this compares with 23.48 percent of control youth. The multivariate analysis confirms that there was no significant impact of STEP participation on suspensions. The estimates were also not statistically different by gender or race/ethnicity; however, there was one significant difference across sites--treatment youth in Seattle were significantly more likely to have been suspended than controls. Almost 36 percent of treatments in Seattle reported having been suspended, compared with only 26 percent of control youth in that site. Discussions with site monitors provided no ready explanation for this difference.

Test Performance

In all sites except Portland, the school district administers standardized tests to ascertain the performance of their

Table VIII.1

THE ESTIMATED IMPACTS OF STEP ON ABSENTEEISM AND TEST SCORES IN READING AND MATH DURING FIRST POSTPROGRAM SCHOOL YEAR BY GENDER, RACE/ETHNICITY AND SITE FOR COHORT I

Group	Percentage of the Year Absent[a]	Reading Scores in NCEs[b]	Math Scores in NCEs[b]
Overall	0.11	0.44	2.60**
Gender			
Male	0.29	0.31	2.65
Female	-0.11	0.57	2.55
Race/Ethnicity			##
Asian	-2.17	-0.65	1.23
Black	0.70	-0.12	4.65***
Hispanic	0.89	3.21	3.32
White & Other	-2.28	0.52	-10.09**
Sites			
Boston	-1.19	-1.73	3.67
Fresno	2.21	0.08	3.46
Portland	-0.62	n.a.	n.a.
San Diego	n.a.	2.08	0.23
Seattle	-0.08	1.58	2.91
Sample Size	819	578	576

n.a. = not available.

Note: Missing data on at least one variable in the regression reduced the sample from the original 1,576 Cohort I youth. In specific regressions, the sample was further reduced because certain sites or individuals were missing data on the dependent variable. Control group means are: percentage of year absent --14.84 percent, reading score--45.42, math score--54.28.

[a] San Diego did not provide absenteeism data.

[b] Portland test score data were omitted because they were not comparable to those in other sites. Only 46 percent of the San Diego youth had test data.

***Indicates that the impact is statistically different from zero at the 0.01 level.

**Indicates that the impact is statistically different from zero at the 0.05 level.

##Indicates that the impact differs with respect to this characteristics at a 0.05 level of significance.

students relative to a national norm.[82] The scaled test scores of treatments and controls for Spring 1987 were converted to normal curve equivalent scores (NCEs) so that data from the different tests used by the four sites would be comparable for the analysis.

Table VIII.1 shows that the standardized math scores of Cohort I treatment youth were significantly higher--by 2.6 points--than those for controls. The impact on math scores, however, varied by race/ethnicity. STEP participation was most effective among black youth, increasing math scores by 4.65 points; it was least effective among whites and others. STEP participation showed no impact on Spring 1987 reading scores for Cohort I youth.

The site-by-site analysis of scaled reading and math scores presented in Table VIII.2 indicates that, with the exception of reading scores in Boston, both math and reading scores of treatment youth were slightly higher than those of controls. In every case, however, the magnitude of the improvement is small and not statistically significant; the largest increase for treatments was 2.16 percentage points in reading scores in San Diego.

Grade Progression

Four indicators of grade progression were examined: highest grade completed, credits earned in SY 1986-87, promotion in SY 1986-87 and graduation. Although all four variables reflect advancement, some are more descriptive of skill acquisition than others. Because school districts vary in their attitudes toward promotion, it is important to consider credit accumulation and graduation as well as promotion rate and highest grade completed if one wants to obtain an accurate picture of STEP's impact on skill acquisition.[83]

Table VIII.3 presents the effects of STEP on these four outcome variables. Although Cohort I treatment youth earned fewer credits than controls, they were more likely to be promoted at the conclusion of school year 1986-87. Only Boston and Seattle, however, provided information on promotion status as of the end of the school year. In these two sites, 16.3 percent of the control youth failed (in other words, 83.7% passed). The estimated promotion impact of 8.57 percentage points in the sites (shown in Table VIII.3) implies that treatment youth are 53 percent less likely to fail than controls (or 10% more likely to pass).

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[82] See Chapter VII for a discussion of the testing done in Portland.

[83] The problems associated with measures of these outcomes are discussed fully in Chapter VII.

Table VIII.2

THE ESTIMATED IMPACTS OF STEP ON SCALED TEST SCORES DURING THE FIRST POSTPROGRAM SCHOOL YEAR BY SITE FOR COHORT I

Site (Standarized) Test Use)	Scaled Reading Scores		Scaled Math Scores	
	Estimated Impact as a Percentage of the Control Group Mean	Sample Size	Estimated Impact as a Percentage of the Control Group Mean	Sample Size
Boston (MAT)	-0.48%	152	1.39%	153
Fresno (CAT)	0.33	152	0.53	149
Portland (PALT)[a]	0.89	193	0.76	194
San Diego (CTBS)	2.16	143	0.97	143
Seattle (CAT)	0.60	134	1.87	134

Note: None of the estimated impacts were statistically different from zero.

[a] In Portland, only individuals who had not already reached a certain minimum level of competency (62% of the Portland sample) were given a test. Thus, the sample in Portland is not comparable to the samples in other sites.

Table VIII.3

THE ESTIMATED IMPACTS OF STEP ON CREDITS EARNED, GRADE, AND PROMOTION DURING THE FIRST POSTPROGRAM SCHOOL YEAR BY GENDER, RACE/ETHNICITY AND SITE FOR COHORT I

Characteristic	Grade Completed	Percentage of Graduation Credits Earned	Probability of Promotion[a,b]	Probability of Graduation[b]
Overall	-0.01	-1.72**	8.57**	0.66
Gender				
Male	-0.03	-1.64	8.36	-0.90
Female	0.01	-2.36*	8.76	1.37
Race/Ethnicity	###			
Asian	-0.17*	-2.27	n.a.	-
Black	-0.05	-2.25*	5.74	-
Hispanic	0.22**	-0.21	10.72	-
White & Other	0.07	-2.93	22.82*	-
Sites				
Boston	0.10	0.87	7.04	-
Fresno	-0.08	-2.78*	n.a.	-
Portland	0.09	-0.14	n.a.	-
San Diego	-0.16*	-5.41***	n.a.	-
Seattle	-0.02	-1.43	22.40	-
Sample Size	1,109	988	424	1,311

n.a. = not available.

Note: On average, controls had completed 11.25 years of school and had earned 17.5 percent of the credits they needed to graduate; 83 percent passed the grade they were enrolled in by the end of the school year (68.8% in Boston and 95.2% in Seattle); and 1.77 percent had graduated.

[a] Only Boston and Seattle reported promotion status as of the end of school year 1986-87.

[b] These impacts were derived from a logit analysis.

***Indicates that the impact is statistically different from zero at the 0.05 level.
**Indicates that the impact is statistically different from zero at the 0.05 level.
*Indicates that the impact is statistically different from zero at the 0.10 level.

###Indicates that the impacts differ with respect to this characteristic at a 0.01 level of significance.

In follow-up interviews conducted in Fall 1987, both treatment and control youth reported that they had completed an average of 11.25 years of school. Only among Hispanic youth had treatments advanced significantly further in school than controls--by roughly a quarter of a grade.

Since only 29 sample members had graduated by the end of SY 1986-87, the graduation results presented in Table VIII.3 must be viewed as tentative. It appears, however, that treatment youth were more likely to graduate than controls. Overall, 1.77 percent of the controls had graduated, compared with 2.43 percent of treatments. STEP participation had its largest impact on graduation rates among female treatments.[84]

Dropping Out

STEP's impact on dropout rates was analyzed using the two measures based on 1986-87 school records that were discussed in Chapter VII. The more restrictive measure counts an individual as a dropout only if school records explicitly show him/her to be a dropout. The less restrictive measure also includes individuals whose status is unknown to the school. Because some youth considered as dropouts by the school may actually have reenrolled in other school districts, this definition probably overstates the rate.

In addition, self-reported data from the follow-up interviews was analyzed. Self-reported data are probably more accurate than the school records data. While schools have an incentive to retain youth on their rolls, youth have no particular reason to misrepresent their status. However, self-reported data do not include information for youth who were not located for the follow-up interview, and since interviews were conducted approximately six months after the conclusion of SY 1986-87, the measure covers a longer time period than the school-based measures. During that interval, additional youth may have dropped out of school, while youth who dropped out during SY 1986-87 may have returned and been enrolled at the time they were interviewed. Nevertheless, because we expect it to fall between the rates estimated by the school-based definitions, we assume that the self-reported measure is closest to the true dropout rate.

Table VIII.4 shows the impact of STEP participation for the sample as a whole and for various subgroups. Overall, fewer treatment youth dropped out than controls, but the impact is not statistically significant. This is not surprising, given the small number of youth who have dropped out. STEP participation reduced the dropout rate by 13 percent (from 27.11% to 23.66%)

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[84] Not surprisingly, given the small number of graduates, none of these results is statistically significant.

Table VIII.4

THE ESTIMATED IMPACTS OF STEP ON DROPPING OUT OF SCHOOL BY THE END OF THE FIRST POSTPROGRAM SCHOOL YEAR BY GENDER, RACE/ETHNICITY AND SITE FOR COHORT I

Group	Self-Reported Dropout	Restrictive School-Reported Definition of Dropout	Less Restrictive School-Reported Definition of Dropout
Overall	-2.57	-1.94	-3.45
Gender		###	##
Male	-3.71	3.32	0.58
Female	-1.36	-7.74**	-8.06**
Race/Ethnicity	#		#
Asian	-5.87	3.49	6.11
Black	1.45	-1.67	-3.13
Hispanic	-9.96**	-1.28	-13.15*
White & Other	-8.38	-6.85	-1.18
Sites		##	#
Boston	-0.96	-5.54	-6.94
Fresno	2.53	-7.06	-10.26
Portland	-9.27**	-7.28	-4.23
San Diego	-2.69	1.91	4.36
Seattle	-2.54	3.65	-1.99
Sample Size	1,113	1,311	1,311

Note: The overall control means are 12.23 percent using the restrictive school-reported dropout rate, 27.11 percent using the less restrictive school-reported dropout rate and 14.24 percent using the self-reported dropout rate.

**Indicates that the impact is statistically different from zero at the 0.05 level.
*Indicates that the impact is statistically different from zero at the 0.10 level.

###Indicates that the impact differs with respect to this characteristic at the 0.01 level of significance.
##Indicates that the impact differs with respect to this characteristic at the 0.05 level of significance.
#Indicates that the impact differs with respect to this characteristic at the 0.10 level of significance.

according to the less restrictive school-based measure and by 18 percent (from 14.24% to 11.67%) according to the self-reported measure.

The school-based measures indicate that STEP significantly reduced the probability that females would dropout of school.[85] For Cohort I females, school-based dropout rates fell approximately eight percentage points according to the restrictive measure--from 13.78 percent to 6.04 percent. According to the less restrictive measure, it also fell by about eight percentage points--from 27.57 percent to 19.51 percent. Unfortunately, the analysis of self-reported dropout status does not confirm the school-based findings. According to the self-reported analysis, STEP participation showed no significant impact on dropout rates for females. In fact, STEP was more effective among males. There is no apparent explanation for this discrepancy.

It is difficult to interpret the meaning of these differential effects on females. On one hand, the school-based results could mean that STEP is generally more effective at keeping females in school than males. Alternatively, they may support the idea that longer-term impacts of STEP will be stronger among groups that showed larger in-program impacts. For Cohort I, STEP participation appeared to have less of an effect on males after the first summer than on females. Analysis of postprogram dropout behavior among Cohort II males, who appeared to benefit from STEP as much as Cohort II females did after their first summer, should help resolve this question.

The impact of STEP participation on dropout rates also appears to vary by race/ethnicity. According to the self-reported dropout status, STEP reduced the probability that Hispanic youth would drop out by 9.96 percentage points--from 24.27 percent to 14.31 percent. This is a decrease of 41 percent and is consistent with the impact on years of school completed; Hispanic treatment youth have also completed more years of school than Hispanic controls. The finding is also consistent with our expectation that postprogram impacts would be strongest among groups of youth who also experienced strong in-program results; in-program impacts on reading and math scores were strongest among Hispanic youth in racial/ethnic subgroup analyses conducted for Cohort I.

Examination of the self-reports indicates that both treatment and control youth cited school-related issues as the most important reason they dropped out of school. Among self-reported dropouts, 53 percent of controls and 41 percent of treatments said they dropped out because they were having problems in school or simply did not like school. Substantially more treatment youth (23%) than controls (13%) reported pregnancy or marriage as the most important reason for dropping out. The remaining youth cited job-related reasons (16%) or such reasons as entering the military, being expelled, illness or family responsibilities. The overall distribution of reasons for dropping out, however, is not statistically different between treatments and controls.[86]

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[85] STEP's impact on males and females differed statistically at the 0.05 level of confidence for both school-based definitions but not for the self-reported measure.

Educational Expectations

The final analysis of STEP's impact on educational outcomes involved changes in youth's educational expectations. At baseline, roughly 71 percent of Cohort I youth reported that they expected to obtain some education beyond high school. At the time of the follow-up interview, this percentage had increased to 74.1 percent among treatment youth and 75.3 percent among controls. Regression analysis confirms that STEP had no impact on educational expectations for the overall sample. However, the subgroup analyses indicated a significant difference between males and females. While STEP participation had no impact on females' educational expectations, treatment males had significantly lower educational expectations at follow-up than control males (by 6.23 percentage points). The lower educational expectations among treatment males may indicate that they are becoming more realistic about their educational prospects.

FERTILITY-RELATED OUTCOMES

In the follow-up interview, youth were asked nine of the 12 contraceptive knowledge items that they had answered on in-program questionnaires. Resulting data were intended to measure the degree to which treatment youth retained the knowledge they gained through STEP. Youth were also asked to provide behavioral information on sexual experience, recent sexual activity, the use of contraceptives, and pregnancy and its outcomes.

Knowledge of Contraception

On average, Cohort I youth correctly answered only about four of the nine questions on contraception at baseline. At the conclusion of the 15-month program, a retest showed treatment youth had significantly increased their knowledge of contraception.[87]

Analysis of responses to the nine-item scale at the follow-up interview for Cohort I indicates that treatment youth correctly answered an average of almost five and a half questions at follow-up, while controls averaged less than four correct responses.

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[86] The chi square is 3.696 with 3 degrees of freedom, which is not significant at even a 0.10 level.

[87] Analysis of the original 12 items found that treatment youth increased their knowledge from an initial average of five correct responses to an average of almost nine correct responses at the conclusion of the second summer.

Results from the multivariate analysis (Table VIII.5) confirm that Cohort I youth have retained more knowledge than control youth. The net impact of STEP on contraceptive knowledge at follow-up is 0.51 (significant at the 0.01 level). Impacts were statistically significant and statistically equal across all gender, racial/ethnic and site subgroups.

Reported Fertility-Related Behavior

Five measures of reported fertility-related behavior were examined--the percentage of youth who have ever had sex, the percentage of youth who had sexual intercourse within the last three months, the frequency of sexual intercourse during the preceding month, the use of contraceptives and the incidence of pregnancy (including responsibility for pregnancies among males).

At the time of enrollment, about 42 percent of Cohort I youth reported that they were sexually experienced. As would be expected with this age group, the percentage of youth who were sexually experienced increased over time. By the first follow-up, 60.3 percent of the control youth were sexually experienced. Table VIII.6 shows that treatments and controls were equally likely to report being sexually experienced. The multivariate analysis confirms that STEP participation had no effect on the probability that Cohort I youth were sexually experienced by follow-up.

Among youth who were sexually experienced, treatments and controls were equally likely to report being sexually active during the three months prior to the follow-up interview. Seventy-eight percent of sexually experienced treatments and 79 percent of sexually experienced controls reported having had sex within the previous three months. Roughly 57 percent of the sexually experienced youth further reported having had sex within the previous four weeks. Among youth who had been sexually active during the previous month (230 treatments and 220 controls), the largest percentage--43 percent of both treatments and controls--reported engaging in sexual activity an average of once or twice a week.

Opponents of sex education often argue that teaching youth about contraceptives and their availability will result in increased sexual activity. These results provide no evidence that the

Table VIII.5

NET IMPACT OF PARTICIPATION IN STEP ON CONTRACEPTIVE KNOWLEDGE FOR COHORT I OVERALL AND BY GENDER, RACE/ETHNICITY AND SITE

Group	Cohort I
	Contraceptive Knowledge[a]
Overall	0.51***
Gender	
Male	0.59***
Female	0.41***
Race/Ethnicity	
Asian	0.58***
Black	0.35***
Hispanic	0.72***
White & Other	0.97***
Site	
Boston	0.42**
Fresno	0.57***
Portland	0.31***
San Diego	0.74***
Seattle	0.51***

Note: The sample size for this analysis is 1,201. Impacts for each subgroup are computed as the sum of the treatment coefficient and each subgroup treatment interaction term coefficient. Significance is then determined by the appropriate F-test of the hypothesis that this sum equals zero. None of the subgroup impacts differed from each other at a 0.10 level.

[a] Mean number of correct responses on a nine-item scale.

***Indicates that the impact differs statistically from zero at the 0.01 level.

**Indicates that the impact differs statistically from zero at the 0.05 level.

Table VIII.6

BASELINE TO FOLLOW-UP CHANGE IN THE PERCENTAGE OF COHORT I YOUTH WITH SEXUAL EXPERIENCE AND USING CONTRACEPTIVES AT LAST INTERCOURSE BY GENDER

	Sexual Experience[a]		Contraceptive Use[b]	
	Baseline	Change	Baseline	Change
Overall				
Treatment	43.3%	18.6***	23.4%	42.9***
Control	40.9	19.5***	24.6	42.4***
Difference		-0.9		0.5
Male				
Treatment	56.2%	11.9***	22.1%	48.7***
Control	55.4	16.0***	26.5	41.7***
Difference		-4.1		7.0
Female				
Treatment	28.1%	26.2***	27.5%	25.5***
Control	23.9	23.6***	17.5	45.0***
Difference		2.6		-19.5

[a] Percentage of youth reporting that they have ever had sex (n = 667 treatments, 655 controls).

[b] Percentage of sexually experienced (at baseline) youth reporting use of contraception at last intercourse (n = 205 treatments, 191 controls).

***Indicates that the change is statistically different from zero at the 0.01 level.

increased knowledge treatment youth demonstrate leads to increased sexual activity. Overall, there was no difference between treatments and controls with respect to any of the measures of reported sexual behavior--sexual experience, recent sexual activity or frequency of sexual intercourse.

About two-thirds of sexually active youth reported that they used contraceptive methods the last time they had sex--66.3 percent of treatments and 67.0 percent of controls. As shown in Table VIII.6, this represents a significant increase over the two years from baseline to follow-up. There was no significant difference, however, in the increase among treatment youth compared with that among controls. The multivariate analysis indicates that treatments were slightly more likely (by 9.8%) than controls to report contraceptive use during their most recent intercourse, but this estimate is also not statistically significant. Similarly, there were no significant program effects on reported contraceptive use for either males or females.[88]

The final fertility-related outcome examined was the reported incidence of pregnancy. Among females, 74 controls (24.1%) and 75 treatments (23.6%) reported that they have become pregnant. Among males, 39 controls (11.0%) and 44 treatments (12.3%) reported that they have been responsible for a pregnancy. The analysis indicates that, as of the follow-up interview, STEP participation had no perceptible influence on either the likelihood of becoming pregnant or being responsible for a pregnancy among Cohort I youth. Logit analyses indicate that treatment females were 11.2 percent less likely to report having been pregnant than controls. Among males, treatments were 16.2 percent more likely to report having caused a pregnancy than controls.[89]

Comparisons of self-reported pregnancy rates with data obtained from vital statistics and program records suggest that adolescents tend to underreport pregnancies, particularly if the pregnancy ended in abortion. Researchers suspect that adolescent males, in particular, underreport pregnancies for which they are responsible. Given the emphasis in the LSO curriculum on male responsibility, treatment males may simply be more likely to report having caused a pregnancy than control males.

The distribution of reported pregnancy outcomes differs by gender and, for females, by treatment status. Among control females, 49 percent of the pregnancies ended in a live birth, while only 33 percent of the pregnancies for which control males reported being responsible ended in a live birth. Conversely, control males reported that about 38 percent of the pregnancies they caused ended in abortion, while only 10 percent of control females' reported pregnancies ended in abortion.

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[88] Small sample sizes within each site and for most racial/ethnic subgroups preclude estimation of impacts.

[89] Neither of these estimates is statistically significant.

Small samples and a differing tendency for control females to refuse to answer these questions make it difficult to determine whether STEP participation affects pregnancy outcomes. (Eighteen percent of controls refused to provide pregnancy outcome information, compared with only 10 percent of treatments.) Among females, treatment status does not seem to affect the reported percentage of live births, but does appear to affect the reported distribution of pregnancies that did not result in live births. Treatments and controls differed in their tendency to report abortions and miscarriages, perhaps because of LSO classes; treatments were more likely to report abortions, while controls were more likely to say that a pregnancy resulted in a miscarriage.

EMPLOYMENT OUTCOMES

Since most STEP youth were still enrolled in school at the time of follow-up, employment consisted primarily of part-time or temporary summer employment. Although employment outcomes will become more important after youth leave school, they are of some immediate interest.

To explore STEP's impact on subsequent summer employment, the analysis focused on employment outcomes during the quarter encompassing the summer following STEP completion (July-September 1987). STEP's overall and subgroup effects on quarterly earnings, employment and average weekly wages are shown in Table VIII.7.

During this period, there was no difference in the percentages of treatment and control youth who were employed. Approximately 73 percent of both groups reported being employed during this quarter. Impacts were also not found for gender, racial/ethnic or site subgroups.

Control youth, however, averaged more weeks on the job than treatment youth, which may reflect the somewhat higher percentage of dropouts among control youth. After controlling for this difference in work experience, the analysis of earnings during this quarter indicates that control youth earned slightly more, on average, than treatment youth; however, the impact is not statistically significant. Similarly, there was no significant difference in average weekly wages earned by treatment and control youth during the quarter. None of the effects differed statistically by gender, race/ethnicity or site.

The last employment-related outcome we examined is the proportion of youth who have been fired from any job since June 1985, when

Table VIII.7

THE ESTIMATED IMPACT OF STEP ON EARNINGS, THE PERCENTAGE EMPLOYED AND WEEKLY WAGE IN QUARTER NINE BY GENDER, RACE/ETHNICITY AND SITE FOR COHORT I

Characteristics	Quarterly Earnings (in dollars)	Percentage Employed	Weekly Wage (in dollars)
Overall	-37.25	-0.22	-5.51
Gender			
Male	-86.62	1.93	-8.68
Female	25.24	-2.48	-1.86
Race/Ethnicity			
Asian	80.97	-9.25	-0.45
Black	-64.28	0.80	-6.43
Hispanic	-113.92	-1.62	-1.44
White & Other	88.53	9.20	-11.88
Sites			
Boston	12.22	-3.65	5.10
Fresno	-95.06	-3.29	3.82
Portland	-25.57	3.56	-18.55**
San Diego	-55.82	-1.14	-12.10
Seattle	-20.22	3.20	-5.92
Sample Size	806	1,114	806

Note: 73.2 percent of controls were employed during this quarter; control youth earned an average of $1,065.90 during the quarter and had an average weekly wage of $124.35. None of the impacts differ statistically by gender, race/ethnicity or site.

**Indicates that the impact is statistically different from zero at the 0.05 level.

Cohort I youth entered STEP. The multivariate analysis indicates that treatment youth were 0.6 percent more likely to have been fired than controls, but the difference was not significant.

SUMMARY OF POSTPROGRAM OUTCOMES

Overall, the postprogram impacts observed reflect the modest in-program impacts observed among Cohort I youth. Given our assessment that STEP was a weaker intervention for this group of participants than it was for the two cohorts that followed, we expect that postprogram impacts will be greater for Cohorts II and III.

The analysis detected some modest postprogram benefits for participants. The pooled analysis of standardized test scores that treatment youth scored higher on math tests taken in Spring 1987. This was particularly true for black treatment youth, who increased their math scores by 4.65 points over controls. In the two sites where promotion data were available, STEP participation increased the probability that a youth was promoted at the end of SY 1986-87.

Although the overall program impact on dropout rates was not statistically significant, more control youth than treatments had dropped out of school by the conclusion of SY 1986-87 and by the follow-up interview. Among Hispanic youth, STEP participation significantly reduced the probability of dropping out of school. In addition, Hispanic treatment youth had completed significantly more years of school than control counterparts by the time of the follow-up interview. Two of the three measures of dropout behavior also indicate that STEP significantly reduced the dropout rate among females.

A year after completing STEP, treatment youth still showed greater knowledge of contraception than did control youth. There were no impacts, however, on any measures of fertility-related behavior. Treatment and control youth were equally likely to be sexually experienced and recently sexually active at the follow-up interview. Thus, contrary to what opponents of sex education may predict, exposure to the Life Skills and Opportunities curriculum did not increase reported sexual activity.

Although treatment youth were slightly more likely to report having used contraceptive methods during their most recent intercourse, the difference between treatments and controls was not statistically significant. There also was no overall impact on the incidence of pregnancy. Female treatment youth were slightly less likely to have been pregnant than controls, while treatment males were more likely to report having been responsible for a pregnancy than control youth. There were differences in reported pregnancy outcomes; primarily, treatment females reported more abortions and fewer miscarriages than control females.

Finally, STEP participation had no impact on the probability that youth were employed or on their earnings during the 1987 summer. Approximately 73 percent of both treatment and control youth reported being employed during the quarter encompassing July through September 1987. Controlling for weeks on the job, treatment and control youth had similar earnings and average wages during the quarter.

IX. COST ANALYSIS

This chapter examines the costs of operating a 15-month STEP intervention that go beyond those of operating a one-summer program that provides work experience only to a similar group of 14- and 15-year-old youth. STEP costs are compared with those of a one-summer work experience program rather than a two-summer work experience so that the program used in the cost analysis will be comparable to the one experienced by the comparison group in the impact analyses.[90] Such consistency is necessary to make appropriate cost/benefit assessments.

A similar analysis of STEP costs was conducted in 1986; however, that was the first year sites reported STEP costs and their data were collected retrospectively. For the 1987 program, costs were frequently documented on an ongoing basis; thus, estimates are more precise and realistic. Moreover, this report includes estimates of not only the incremental costs of STEP to society, but the incremental out-of-pocket costs to the program as well. The major difference between these two types of costs is that social cost includes the value of donated goods and services but not the wages paid to the youth, while out-of-pocket cost does not include the value of donated goods and services but does include wages.

Examination of the demonstration costs shown in Table IX.1 reveals that the average cost to society of providing one summer of work experience to approximately 150 14- and 15-year-olds is $674 per enrollee. (The average out-of-pocket cost is $838 per enrollee.) The additional social cost of providing a summer of remediation and LSO training along with work experience is $487 per enrollee ($580 out-of-pocket). After their first summer of STEP, program youth receive support services during the school year and a second summer of work experience, remediation and LSO. Control youth receive no additional services. When the cost of providing these extra services are added to the first summer's net cost, the cost of 15 months of STEP comes to $1,685 per enrollee in social cost, or $1,947 out of pocket.[91]

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[90] Control group youth in most sites are guaranteed only one summer of work experience. Few 14- and 15-year-olds attempt to secure or succeed in securing a second summer SYETP placement, mostly because the number of SYETP jobs are limited and are generally reserved for older youth.

[91] The $1,947 figure includes $669 in wages per youth that were incurred in the second summer when control group youth receive no SYETP wages. However, the $1,947 does not include $407 of donated goods and services per youth that are included in the social cost figure.

Table IX.1

SUMMARY OF STEP COSTS

	SOCIAL COST PER ENROLLEE			OUT-OF-POCKET COST PER ENROLLEE		
	One-Summer Program, Work Experience Only	Incremental Cost of a 15-Month STEP Program	Total STEP Cost	One-Summer Program, Work Experience Only	Incremental Cost of a 15-Month STEP Program	Total STEP Cost
Boston	734	1,389	2,123	654	1,712	2,366
Fresno	735	1,140	1,875	999	1,532	2,531
Portland	548	1,854	2,402	706	1,849	2,555
San Diego	703	1,730	2,433	796	2,101	2,897
Seattle	651	1,820	2,471	1,036[a]	2,100	3,136
Average (Including P/PV Expenses)	674	1,685	2,359	838	1,947	2,785

[a] Seattle's work experience program cost more than other sites because they extensively used paid crew leaders as supervisors, while supervisors of individual youth in other sites did not charge the program for supervisory time.

The following sections break down those cost estimates by component, examining component costs incurred in each site and variations across sites. Each component section concludes with our best estimate of the component cost for a STEP replication. Before presenting the results, the analytical framework for the cost analysis is explained, including discussions of the comparison group used, the method used to derive 15-month cost estimates from 12 months of cost data, the perspective from which costs were examined, the method by which several complications that arose during the analysis were handled, and the data sources that were used.

METHODOLOGY

While an analysis of program costs is straightforward in some respects, several conceptual issues arise in providing accurate estimates. These are discussed in this section.

Comparison Group

As mentioned earlier, the costs presented in this analysis are the incremental costs of providing a STEP intervention as opposed to one summer of work experience only. The one-summer comparison program is used rather than the two-summer comparison program that was used for the 1986 analysis[92] so that the comparison program is consistent with the control group used for the impact analyses, thus enabling comparison of the impacts of STEP with the net cost.

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[92] In the 1986 report, incremental costs were those over and above providing two summers of work experience. The comparison program for that cost analysis was a two-summer work experience program because only costs over and above serving controls were collected. We were unable to estimate the cost of running STEP without a parallel work experience program. This year, separate treatment and control costs were documented in all sites except Boston. Thus, we constructed the appropriate incremental cost for cost/benefit purposes--the cost of a 15-month STEP program over and above a one-summer work experience program. For other purposes, the reader may wish to know the cost of STEP relative to a two-summer work experience program. For the 1987 program, this alternative social cost is the net cost of providing a summer of STEP's curriculum over and above work experience in the first summer, $487 per enrollee; the cost of school-year support, $309 per enrollee; reenrollment costs, $62 per enrollee; and the net cost of providing a second summer of STEP curriculum over and above work experience, $487 for those who return for a second summer. Only 73 percent of the youth return for a second summer; thus, the expected 15-month social cost of STEP over and above a two-summer work experience program is $1,215.

The comparison program used in this report also differs from that used in the 1986 report in that the cost of determining educational eligibility is not counted as a net STEP cost. Legislation passed in 1986 requires SYETP to assess all participants' need for remediation (though it does not specify which services should be given to the needy youth). Thus, the costs of the comparison program include educational eligibility costs.[93]

Synthetic (Hypothetical) Cohort Technique

The major task in the STEP cost analysis is determining the cost of providing a full 15 months of STEP services to youth. One way to calculate this cost is to total the costs incurred in serving Cohort II youth during Summer 1986, SY 1986-87 and Summer 1987. A second approach is to calculate a 15-month cost from the costs that sites incurred by providing school-year support, remediation, work experience and general program administration to both Cohorts II and III from the end of Summer 1986 to the end of Summer 1987. The second approach was chosen for several reasons. First, cost estimates for 1987 should be more precise than the 1986 estimates because the use of staff timesheets improved the accuracy of cost accounting. Second, because a new curriculum was introduced during the summer of 1986, the costs for that summer might be higher than would be the case in an established STEP program. Third, sites do not record or report costs separately by cohort, which would be necessary to determine STEP costs using the first method.

Fifteen-month costs are derived from 12 months of data by first carefully determining the cost of each major component of STEP--pre-summer activities, summer instruction, life skills training, work experience, SYS, reenrollment and administration. The average component cost per youth is derived by dividing each component cost by the number of youth receiving that component. For example, the per-enrollee cost of reenrollment is calculated by dividing total reenrollment costs by the number of youth in Cohort II who were guaranteed a job in the second summer (all Cohort II treatments and Cohort II controls in San Diego and Seattle). The per-enrollee cost of remediation is calculated by dividing total remediation costs by the number of Cohort III treatments and returning Cohort II treatments. After ascertaining the per-enrollee cost of each component, the costs are aggregated as they would occur over 15 months, as shown in Table IX.2. This method assumes that the per-enrollee costs are the same in the first and second years for activities that occur in both summers, even though costs in the second year are incurred only for those who actually return.

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[93] Educational eligibility costs are low ($11 per enrollee). A comparison program that did not incorporate an eligibility criterion would change the cost estimate little.

Table IX.2

THE SYNTHETIC COHORT APPROACH:
SOURCES OF COMPONENT COSTS

Components	Source
First-Summer Costs (Remediation, LSO and Work Experience)	Costs of serving Cohort III treatments and returning Cohort II treatments in 1987
School-Year Support	Costs of serving Cohort II in SY 1986-87
Reenrollment	Costs of reenrolling Cohort II in 1987
Second-Summer Costs (Remediation, LSO and Work Experience)	Costs of serving Cohort III treatments and returning Cohort II treatments in 1987
Administrative Costs	Other costs of serving Cohorts II and III between September 1986 and August 1987

Finally, when calculating per-enrollee costs, fixed costs were not separated from other costs. Thus, all per-enrollee costs assume that approximately 150 youth enroll in the program each year and approximately three-quarters return for a second summer.

Cost Perspective

The costs, as well as benefits, of a program like STEP can be analyzed from a variety of perspectives, including the government's, the program's and society's. The most common approach in cost/benefit evaluations is to analyze program costs from the perspective of society. This approach focuses on the overall value of goods and services society invests in the program, regardless of whether they are provided in kind or purchased. From the societal perspective, "transfer payments," i.e., changes in the ownership of resources by groups within the society, are not counted as costs. For example, STEP participants' wages represent a cost to one group (usually the government), but are offset by an equal benefit to another group (the participants). Only changes in the total value of resources are counted as costs from society's perspective--for example, the operational expenses involved in making transfers. The societal perspective is presented in this analysis because it represents the most appropriate approach to examining the economic efficiency of the program.

However, program operators must also know the expected out-of-pocket expenses. The social cost estimate does not provide this information because social cost includes the value of all donated goods and services but not the transfer payments made to participants. Therefore, costs are also discussed from the program's perspective.[94]

The costs that a particular program will incur when replicating STEP could differ from those presented in this chapter, however, since sites' ability to obtain donations will vary. Also, when STEP is perceived as a permanent program, some goods and services that once were donated may no longer be provided free of charge. For example, SYETP provided STEP free office space in some sites. Rent would likely be assessed to a permanent STEP program, changing the balance between donated goods and services and out-of-pocket expenses. In addition, new sites will incur costs associated with starting a new program. Still, the costs presented in this report will be valuable to program operators in assessing possible costs of implementing STEP.

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[94] Costs from the government's perspective are not reported because societal and government costs are very similar in this case. They differ only insofar as the youth pay taxes, which few do.

Cost Evaluation Issues

Before tallying costs, a number of issues that affected the final cost estimates had to be resolved. These included defining the steady-state costs, disentangling operational costs from research and demonstration costs, valuing contributed goods and services and treating joint costs.

Cost of Steady-State Operations

Establishing a STEP intervention entails considerable development costs, particularly those involved in creating the remediation and LSO curricula. These costs will diminish once the program is in full operation; thus, they should not be included in steady-state costs, which are the costs that are likely to prevail in the long run. Since the nature of demonstrations dictates that a relatively large proportion of costs will be either start-up or phase-down costs, analysts determining steady-state costs based on demonstration experiences must be particularly wary. Thus, we needed a method of excluding the amortized value of start-up and phase-down expenditures in the STEP demonstration from our cost estimates.

There are two methods of isolating the steady-state costs of a program: either start-up and phase-down costs must be quantified and omitted, or costs must be observed during a period following initial start-up but before the beginning of the phase-down. The latter approach was used here; the cost analysis covers costs incurred during 1986-87. In all sites except Boston, which was also a pilot site, this period covers the third of four summers of program operations and the second of three school years. Thus, for the most part, start-up and phase-down activities are not addressed.

The 1987 cost estimates are more indicative of steady-state costs than the 1986 estimates largely because P/PV developed and the site adopted a new remediation curriculum during 1985-86, the period used in the previous cost analysis. Thus, the 1987 cost estimates are indicative of the cost of expected year-to-year curriculum modifications rather than major curriculum development.

The 1987 cost estimates also provide a better estimate of the cost of the SYS program, which was not as active in SY 1985-86 as in SY 1986-87, nor as it would probably be in a steady-state program. Thus, 1987 cost estimates for SYS are more likely to indicate steady-state costs.

Operational Versus Research/Demonstration Costs

Another common problem in evaluating program costs in a demonstration setting is excluding costs associated with the demonstration's research requirements. These are costs incurred because the intervention is the focus of special research inquiry or because more intensive efforts are needed to implement an intervention that will enable extrapolation of findings to other populations. These are purely demonstration costs; one example is the cost of testing the control group.

For the most part, the hourly expense of P/PV's local site coordinators is also a demonstration cost, since the coordinators' primary functions are monitoring and documenting the implementation of the program, which are clearly research activities. The coordinators also provide technical assistance to ensure the smooth operation of the program, but one could argue that these activities also represent demonstration costs. In individual cases, however, the involvement of site coordinators went beyond their demonstration-specific roles. Taking a conservative approach to this assignment process, site coordinator costs were considered to be operational only if they clearly and unmistakably went beyond what was expected as part of the demonstration, such as providing teacher training or serving as an interim SYS coordinator.

Purchased Versus Donated Goods and Services

A cost analysis from society's perspective must account for the value of goods and services that are contributed as well as those paid for with program funds. This is important because had donated resources not been used for program purposes, society could have otherwise used them--in other words, there is an opportunity cost. However, while the need to include contributed goods and services is clear, estimating their value is often more difficult. Because they may not appear in formal accounting records, donated goods are frequently overlooked or misestimated. Respondents frequently have a difficult time recalling resources that, though vital to the program, are not a formal part of the program budget.

Determining the opportunity cost of the resource is also problematic, since its potential for alternative use may be in question. For example, donated classroom space and computers may have sat idle for the summer had they not been used by STEP.

For this analysis, donated goods and services are valued at their full market price. Thus, the social cost estimate indicates the maximum cost a program would incur if donations were not obtained, and the cost of donated goods and services are excluded from the out-of-pocket cost estimate.

Treatment of Joint Costs

Another decision that must be made in most cost analyses is how to treat joint costs. Joint costs for this analysis are those that either pertain to more than one program or to more than one program component. Joint program costs are the more serious problem because if they are not allocated appropriately, an under- or overestimation of STEP costs will result. An example of a joint program cost is the funding of services provided to both STEP participants and control group members who are SYETP enrollees. Joint program costs, such as job development, were incurred in a few instances during the demonstration and were allocated as carefully as possible.

Separating costs that pertain to more than one component of a single program is not as crucial in determining overall program cost. Determining the cost of each component, however, is important when using the synthetic cohort approach. Care was taken to correctly separate these costs.[95] Separating program administration costs from the costs of delivering remediation and life skills proved to be particularly difficult.

Data Sources and Issues

The primary sources of cost data were accounting and other program records kept by the lead agency at each STEP site, as well as by other agencies that were involved in operations, such as school districts.[96] P/PV site coordinators were responsible for identifying the appropriate records and key staff members in each organization, distributing timesheets, obtaining the needed information for the relevant time period, and conducting a preliminary analysis of the information to assess its accuracy and determine whether adjustment or revision was needed.

The maintenance of timesheets in 1987 constituted a vast improvement over the 1986 cost-recording system. In 1986, neither site coordinators nor local agency staff maintained detailed records on program costs during the actual period of program operation. Instead, the data collection effort was retrospective and was therefore constrained by the extent of information retained. In 1987, key individuals recorded the time and resources spent on STEP, improving the quality of the data. Although the consistency of these timesheets varied by site and by individual, the overall accuracy of 1987 cost estimates is much higher than that of the 1986 cost estimates.

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[95] Variation in component cost across sites may still be due in part to different site allocations of costs among components.

[96] Sites submitted budgets for the funds P/PV provided to them. However, because these were not the only resources used in STEP, these budgets are not sufficient for the cost analysis.

THE COSTS OF STEP

This section presents the costs for each major component of STEP and explores variations by site. Variations are discussed in terms of social costs, as shown in Table IX.3, rather than out-of-pocket costs, as shown in Table IX.4, because most variations in out-of-pocket costs were due to site differences in donations. Also, per-enrollee costs are presented rather than total costs in order to adjust for differences in program size across sites, which were primarily due to variations in the number of returning Cohort II treatments. Table IX.3 displays first-summer, SYS, reenrollment and second-summer costs separately. For STEP components that are offered in both summers, costs are assumed to be the same for each. Thus, site variations for these components are discussed in terms of first-summer costs. Costs that differ in the first and second summers, such as work experience, are discussed separately. The discussion of each component ends with our best estimate of the cost of replicating STEP.

First-Summer Costs

First-summer costs have been calculated in each site for remediation, LSO, work experience, and administration and support services. Per-enrollee costs for remediation and LSO are assumed to be equal for both summers. For the other components, first-summer and second-summer costs are assessed separately.

Remediation

Remediation costs are divided into two categories: summer instruction, which constitutes the bulk of remediation costs, and pre-summer activities. Pre-summer activities include curriculum development and modification, and staff hiring and training.

Summer Remediation Instruction. A major cost of operating STEP, over and above running a work experience program, is the cost of instruction. Considering the costs in all sites and the cost of P/PV-provided technical assistance,[97] the total instructional cost per enrollee was approximately $514 for each summer. No second-summer costs were incurred for the 27 percent of Cohort II treatments who did not return.

Summer instruction costs varied widely from site to site during 1987, ranging from Fresno's $289 per enrollee to Seattle's $590 per enrollee. Much of the variation among sites is accounted for by differences in instructors' salaries and benefits, factors that are beyond the control of the program. Instructors in Fresno received the lowest hourly wages while Seattle received

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[97] P/PV provided the sites with remediation technical assistance valued at $14,733.

Table IX.3

ESTIMATED NET SOCIAL COSTS PER COHORT MEMBER BY SITE FOR 15 MONTHS OF STEP

Program Component	Boston	Fresno	Portland	San Diego	Seattle	Replication Estimate[a]
First-Summer Costs						
Remediation						
Summer Instruction	522	289	522	589	590	514[b]
Pre-Summer Activities	90	60	27	7	66	74[b]
Total	612	349	549	596	656	588
Life Skills & Opportunities	72	43	76	86	102	96[b]
Work Experience						
Job Development & Placement	14	-30	5	60	-1	10
Worksite Supervision	-315	-271	-124	-293	-253	-251
Total	-301	-301	-119	-233	-254	-241
Program Administration and Support Services	21	5	62	69	65	44
Total First-Summer Costs	404	98	569	518	609	487
School-Year Support Costs	222	360	507	284	170	309
Reenrollment and Eligibility	82[c]	62	13	84	68	62

Note: All entries were rounded to the nearest dollar.

n.a. = not available.

[a] In all cases, the replication estimate is the average demonstration cost, including P/PV-incurred costs.

[b] These figures include P/PV-incurred costs of $14,733 for summer instruction, $31,310 for pre-summer activities and $25,579 for LSO. These costs include those for curriculum development, teacher training and technical assistance.

[c] The cost of determining economic eligibility in Boston is assumed to equal the average in the other sites, $37.

[d] The cost of program administration and support services in Boston is assumed to equal the average in the other sites, $102.

[e] Expected 15-month costs, averaged over the first-summer enrollees, are the sum of the first summer, school-year support and reenrollment costs plus second-summer costs in proportion to the percentage of youth who returned.

Table IX.3 - (cont'd)

ESTIMATED NET SOCIAL COSTS PER COHORT MEMBER BY SITE FOR 15 MONTHS OF STEP

Program Component	Boston	Fresno	Portland	San Diego	Seattle	Replication Estimate[a]
Second-Summer Costs						
Remediation						
Summer Instruction	522	289	522	589	590	514[b]
Pre-Summer Activities	90	60	27	7	66	74[b]
Total	612	349	549	696	656	588
Life Skills & Opportunities	72	43	76	86	102	96[b]
Work Experience						
Job Development & Placement	20	39	23	92	27	40
Worksite Supervision	315	311	292	299	295	303
Total	335	350	315	391	322	343
Program Administration and Support Services	n.a.	76	120	85	126	102
Total Second-Summer Costs	1,121[d]	818	1,060	1,158	1,206	1,129
Expected 15-Month Costs Per Original Enrollee[e]	1,389	1,140	1,854	1,730	1,820	1,685

Note: All entries were rounded to the nearest dollar.

n.a. = not available.

[a] In all cases, the replication estimate is the average demonstration cost, including P/PV-incurred costs.

[b] These figures include P/PV-incurred costs of $14,733 for summer instruction, $31,310 for pre-summer activities and $25,579 for LSO. These costs include those for curriculum development, teacher training and technical assistance.

[c] The cost of determining economic eligibility in Boston is assumed to equal the average in the other sites, $37.

[d] The cost of program administration and support services in Boston is assumed to equal the average in the other sites, $102.

[e] Expected 15-month costs, averaged over the first-summer enrollees, are the sum of the first summer, school-year support and reenrollment costs plus second-summer costs in proportion to the percentage of youth who returned.

Table IX.4

NET OUT-OF-POCKET COSTS PER COHORT MEMBER BY SITE FOR 15 MONTHS OF STEP

Program Component	Boston	Fresno	Portland	San Diego	Seattle	Demonstration Average (Including P/PV Costs)
First-Summer Costs						
Remediation						
Summer Instruction	397	193	348	488	528	402[a]
Pre-Summer Activities	90	37	21	7	65	68[a]
Total	487	230	369	495	593	470
Life Skills & Opportunities	63	40	47	84	98	86[a]
Work Experience						
Job Development & Placement	14	-30	5	59	-2	9
Worksite Supervision	0	0	14	1	-154	-28
Wages	55	-13	28	-14	-39	3
Total	69	-43	47	46	-195	-16
Program Administration and Support Services	21	8	46	69	58	40
Total First-Summer Costs	640	235	509	694	554	580
School-Year Support Costs	222	327	487	284	170	298
Reenrollment and Eligibility	82[b]	38	12	84	68	57

Note: All entries were rounded to the nearest dollar.

n.a. = not available.

[a] These figures include P/PV-incurred costs of $14,733 for summer instruction, $31,310 for pre-summer activities and $25,579 for LSO. These costs include those for curriculum development, teacher training and technical assistance.

[b] The cost of determining economic eligibility in Boston is assumed to equal the average in the other sites, $37.

[c] The cost of program administration and support services in Boston is assumed to equal the average in the other sites, $102.

[d] Expected 15-month costs, averaged over the first-summer enrollees, are the sum of the first summer, school-year support and reenrollment costs plus second-summer costs in proportion to the percentage of youth who

Table IX.4 - (cont'd)

NET OUT-OF-POCKET COSTS PER COHORT MEMBER BY SITE FOR 15 MONTHS OF STEP

Program Component	Boston	Fresno	Portland	San Diego	Seattle	Demonstration Average (Including P/PV Costs)
Second-Summer Costs						
Remediation						
Summer Instruction	397	193	348	488	528	402[a]
Pre-Summer Activities	90	37	21	7	65	68[a]
Total	487	230	369	495	593	470
Life Skills & Opportunities	63	40	47	84	98	86[a]
Work Experience						
Job Development & Placement	20	39	21	92	24	39
Worksite Supervision	0	0	38	7	100	29
Wages	606	846	587	663	630	666
Total	626	885	646	762	754	734
Program Administration and Support Services	n.a.	67	101	85	111	91
Total Second-Summer Costs	1,267[c]	1,222	1,163	1,426	1,556	1,381
Expected 15-Month Costs Per Original Enrollee[d]	1,712	1,532	1,849	2,101	2,100	1,947

Note: All entries were rounded to the nearest dollar.

n.a. = not available.

[a] These figures include P/PV-incurred costs of $14,733 for summer instruction, $31,310 for pre-summer activities and $25,579 for LSO. These costs include those for curriculum development, teacher training and technical assistance.

[b] The cost of determining economic eligibility in Boston is assumed to equal the average in the other sites, $37.

[c] The cost of program administration and support services in Boston is assumed to equal the average in the other sites, $102.

[d] Expected 15-month costs, averaged over the first-summer enrollees, are the sum of the first summer, school-year support and reenrollment costs plus second-summer costs in proportion to the percentage of youth who returned.

the highest, with the difference being approximately $10 per hour.

Non-labor costs were primarily incurred for computers and space, but they also include costs for resource materials, printing and equipment. Although computers and space were frequently donated by the school districts, their use represents a cost from the social perspective because they could have been used for another purpose. Non-labor costs ranged from $96 per enrollee in Fresno to $240 per enrollee in Portland. Variations are due almost entirely to differences in local costs for computers and classroom space.

The 1987 cost of summer remediation instruction was comparable to the 1986 cost in all but two sites--Fresno and San Diego. Costs in both sites were lower in 1987 because estimated non-labor costs were lower. In both sites, more accurate (and lower) rental prices for computers were ascertained in 1987. Also, the San Diego site used almost twice as many computers in 1986 as it did in 1987. Finally, the reported printing costs in Fresno were reduced considerably in 1987.

Pre-Summer Activities. Pre-summer activities include curriculum development and modification, and hiring and training teachers. Curriculum development and modification may seem to be a start-up cost, which should therefore be omitted from the cost estimate for a steady-state program. Even in an established program, however, there will always be some curriculum modification and perhaps even development of new learning modules that reflect changes in the youth's interests. Thus, some curriculum development is included in the cost estimate. As stated earlier, the curriculum development cost incurred in 1987 is more indicative of steady-state costs than the cost for 1986, when a new curriculum was created and sites developed local modules and customized P/PV modules.

Table IX.3 shows that average pre-summer activity costs were $74 per enrollee,[98] but site variations were large. Boston spent the most--$90 per enrollee--primarily because their curriculum development team was larger, included more senior individuals, and was more active than those in other sites. At the other end of the spectrum, San Diego developed a new module, but spent only $7 per enrollee.

The average total cost of providing one summer of remediation for the demonstration (combining pre-summer and instructional activities) was $588 per enrollee. This is also our best estimate of replication cost, since there is no reason to expect future costs in these sites to differ substantially from the 1987 cost.

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[98] The figure includes $31,310 of P/PV-incurred costs for curriculum development and teacher training.

Life Skills and Opportunities (LSO)

The additional cost of providing LSO along with remediation was fairly small, averaging $[illegible]6 per enrollee per summer.[99] Costs ranged from $43 per enrollee in Fresno to $102 per enrollee in Seattle. Even more than with remediation, LSO teachers' salaries constituted the vast majority of component costs. Thus, site variations are almost entirely accounted for by differences in their salaries. Our best estimate of the replication cost per participant for one summer of LSO is the demonstration average of $96.

First-Summer Work Experience

The 1987 cost estimates for the work experience component are much more accurate than the 1986 estimates for two reasons. First, in 1986, sites were only asked to provide information on job development and job placement costs because worksite supervision costs were considered too difficult to obtain retrospectively. In 1987, worksite supervision costs were included. Second, only information on the difference in work experience costs between treatment and controls was collected in 1986. In 1987, sites reported costs for treatment and controls separately, enabling calculation of the cost of providing a one-summer work experience program for 14- and 15-year-olds as well as an estimation of the net difference in costs between the one-summer program and a 15-month STEP program.[100]

The assessment of the costs and benefits of STEP in this report considers STEP relative to a one-summer work experience program for 14- and 15-year-old youth. Table IX.1 presents the site costs of a work experience-only program. The social cost of this type of program is remarkably similar across sites, averaging $674 per enrollee. Out-of-pocket expenses, which include payments to youth but not the donated supervisory time of employers (which is substantial), were $838 per enrollee.

Out-of-pocket costs vary more substantially by site than the social costs, ranging from $654 in Boston to $1,036 in Seattle. Boston's costs are lower than average because little or no job development took place and youth were placed by neighborhood organizations. Seattle's costs are higher than average because the work experience-only program used crew leaders paid by the program to a much greater extent than did other sites. Placements in the other sites were primarily with individual employers who did not charge the program for their supervisory time.

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[99] The $96 average was derived from a total that includes $25,579, that was incurred by P/PV to modify the LSO curriculum and provide technical assistance.

[100] In the 1986 report, only the net difference between STEP and a two-summer work experience program could be determined.

The incremental costs of providing part-time positions for STEP youth, as opposed to full-time positions, are shown in Table IX.3. Job development and placement costs are reported separately from worksite supervision costs.

During the first summer, the cost of providing work experience to the treatment group is substantially lower than that for the control group. Because the STEP jobs are half-time rather than full-time positions, employers spend substantially less time supervising the STEP youth. When supervisory time is taken into account, the work experience costs for the treatments are much lower than those for the controls--an average of $241 less per enrollee. The supervisory costs vary by site, depending primarily on the proportion of crew leaders used in each site for the treatment versus the controls. (Crew leader supervision generally proved to be more expensive than supervision for individual placements.)

The incremental cost of job development and placement is relatively small, averaging $10 per enrollee. Among sites, this cost ranges from $60 per enrollee in San Diego to negative $30 per enrollee in Fresno. A negative figure indicates that it is more costly to develop jobs for and place control youth. In Fresno, it was reported that the total cost of job placement and development was the same for treatments and controls. However, since there were 30 percent more treatments than controls (because more Cohort II treatments returned for a second summer), the per-enrollee costs for controls were larger than for treatments, resulting in the estimated negative net difference. The cost in San Diego was so large because it reportedly took six hours to develop a part-time job but only 1.5 hours to develop a full-time job. The difference between these figures is much greater in San Diego than at any other site; in fact, many sites reported little difference. The difference at San Diego is likely to decline as employers become accustomed to using part-time youth and job developers become more experienced in developing part-time jobs.

Our best estimate of net first-summer job development and placement costs for STEP replication is $10 per enrollee. The replication estimate for worksite supervision is -$251 per enrollee; the estimate is negative because it costs more to supervise full-time controls than half-time treatments. Thus, the overall net work experience cost of STEP for the first summer is estimated at -$241 per enrollee.

Other Services and Administration in the First Year

This category encompasses enrollment activities, program administration and several support functions ancillary to the primary intervention. During the first summer of enrollment, only the costs of operating STEP that go beyond those of operating a work experience-only program are included in general program administration and support services. Administrative costs include those associated with managing summer school teachers, payroll and enforcement of STEP's rules (particularly the "no school, no work" rule); support services include counseling and transporting students to the worksite.

Three of the five sites reported general administrative costs of between $60 and $70 per enrollee. Boston and Fresno had much lower administrative costs--$21 and $5, respectively. In Boston, where youth worked at the same site in which classes were held (the campuses), only one timesheet was needed. This cut down on timesheet collection and payroll processing time. In all other sites, youth filled out both a remediation timesheet and a work timesheet. Although collection of the remediation timesheets was easy, collection of the workplace timesheets entailed driving to many worksites.

In Fresno, net administrative costs were low because the administrative cost of providing a work experience-only program was already relatively large. Fresno used the same number of orientation and payroll personnel to serve both the treatments and controls. Because there were more treatment youth than control youth, the per-enrollee cost of orientation and general payroll administration was smaller for the treatments than for the controls. Thus, while sanctioning costs were much greater for treatments, net administrative costs do not differ very much between treatments and controls.

Because there are no compelling reasons to adjust the cost figures, our replication estimate of the social cost of program administration is the demonstration average of $44 per enrollee, assuming 150 new participants enroll each year. Our best estimate of the net incremental social cost for the first summer of STEP is $487 per enrollee, also assuming an annual cohort of 150 youth.

School-Year Support

As in 1986, all sites were provided with grants from P/PV to operate a SYS program in 1986-87. However, unlike SY 1985-86, all sites were fairly active in SY 1986-87. On average, the 10 months of SYS cost $309 per enrollee. Site costs ranged from $170 per enrollee in Seattle to $507 per enrollee in Portland. Seattle's costs were the lowest because their SYS program was the least active. Conversely, Portland reported the most staff hours among the five SYS programs. The Portland mentors spent more time trying to interact with the youth, and the STEP coordinator was also the full-time SYS coordinator.

When STEP is replicated, the level of activity is likely to vary in much the same way as was seen in the demonstration sites. Thus, our best replication estimate is the demonstration average of $309 per enrollee.

Reenrollment

For youth to reenroll for a second summer in most sites, their economic eligibility must be redetermined. This reenrollment process represents a major administrative cost. In most sites, the cost of reenrollment was between $60 and $85 per enrollee. In Portland, however, STEP was defined as a 15-month program in which youth are continuously enrolled, eliminating the need for recertifying JTPA eligibility.[101] Youth were asked only to sign a letter of intent to reenroll. As a result, reenrollment costs in Portland were only $13 per enrollee, far below those in other sites. Assuming that enough other sites will be able to follow Portland's lead, the replication estimate is set at the demonstration average of $62 per enrollee.

Second-Summer Costs

The incremental cost of running STEP in youth's second summer of participation is much greater than in the first summer because control youth do not receive SYETP summer jobs. While the costs of providing remediation and the life skills classes are the same (since controls do not receive these services), the incremental costs of providing work experience and program administration are substantial. In this section, costs for the second summer are discussed.

Second-Summer Work Experience

The average cost of locating and placing returning youth in half-time jobs, and our replication estimate, is $40. Site variations in job development and job placement costs were similar to those reported for the first summer. San Diego's cost was substantially higher than the other sites' because they estimated that it took six hours to develop and place each youth--by far the highest estimate reported among the sites.

The average cost of supervising the youth at work is $303 per returning enrollee. In the first summer, the cost of supervising the treatments relative to the controls is negative because controls work full time while treatments work only half time. During the second summer, however, it is assumed that the controls are not in a work experience program and thus incur no supervision costs. Therefore, the full cost of supervision during the second summer is counted in the incremental STEP costs.

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[101] To achieve continuous enrollment, youth were enrolled concurrently in Title II-A and Title II-B programs. School-year support constituted the Title II-A program.

Our best estimate of the cost of providing work experience during the second summer for those who return is $343, the demonstration average.

Program Administration

The cost of program administration is also higher in the second summer because there is no parallel work experience program for the controls; thus, the full cost of operating STEP in the summer is counted in the incremental cost. The average program administration cost is $102 per returning enrollee, based on site costs ranging from $76 in Fresno to $126 in Seattle. Salary differences account for most of the site variation. The average cost of providing second-summer components for a returning enrollee is $1,129.

TOTAL 15-MONTH NET COSTS

The average net social cost of enrolling a youth in a 15-month STEP program, beyond the cost of providing one summer of work experience, is $1,685.[102] This includes the costs of the first summer, SYS, reenrollment and a prorated cost for the second summer. Full second-summer costs are not included because they are incurred only for youth who return.[103] Three sites had 15-month costs of between $1,700 and $1,900 per enrollee. Boston and Fresno had lower 15-month per-enrollee costs of $1,389 and $1,140, respectively. To a great extent, Fresno's and Boston's costs were lower due to low summer program costs. However, Boston's 15-month costs were also low because only 61 percent of youth returned for a second summer, compared with the overall demonstration average of 73 percent.

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[102] Had the cost estimate been derived the same way as it was in 1986 (namely, relative to a two-summer work experience program and not including worksite supervision), the 15-month demonstration average would have been $1,525. The 1986 estimated average was $1,587.

[103] The prorated cost per enrollee in each site for the second summer is derived by multiplying the first-summer cost by the proportion of the cohort who returned for a second summer in that site. The rate of return varied, ranging from 61 percent in Boston to 81 percent in Seattle.

Out-of-pocket costs are greater than social costs because the former include the youth's wages. The average out-of-pocket net costs were $1,947 per enrollee, of which $666 represented youth's second-summer wages. The out-of-pocket net costs ranged from $1,532 in Fresno to $2,100 in San Diego and Seattle. The general pattern of costs among the sites mirrors the social cost estimates--Boston's and Fresno's costs were lower than those of the other three sites.

OTHER COST ESTIMATES

As discussed at the beginning of this chapter, all cost estimates presented in this report are relative to the cost of a one-summer work experience-only program for 14- and 15-year-olds. From society's perspective, the average cost of this type of work experience program is $674 per enrollee. The out-of-pocket costs average $838 per enrollee. By adding these base costs to STEP's net cost estimates, the total social cost of providing STEP is estimated to be approximately $2,359 per youth. The total out-of-pocket cost is approximately $2,785 per enrollee.

Two other cost estimates are also of policy interest--the net cost of running a one-summer STEP program and the net cost of running a one-summer STEP program with one year of SYS. The analysis indicates that the incremental cost of running one summer of STEP over and above a work experience program would be $487 per enrollee from society's perspective or $580 from the program's perspective. Net costs of a one-year program would be $796 from society's perspective or $878 from the program's perspective.

It is difficult to assess the effectiveness of a one-summer or one-year program relative to 15 months of STEP because we do not have follow-up data on such programs. Results from an analysis of intervening school-year outcomes for Cohort I (described in the 1986 report) indicated that one summer of STEP followed by a meager SYS program produced little measurable short-term effect on school performance; a similar analysis for Cohort II (described in Chapter VII) indicated that one summer of STEP followed by a more active SYS program also had little effect on school performance. Results from the analysis conducted on the postprogram school-year outcomes for Cohort I (discussed in Chapter VIII), however, indicated that treatment youth were more likely to be promoted and less likely to drop out than control youth. These results provide some evidence that the second summer of services is important. Thus, while a one-summer or one-year program would be less expensive than 15 months of STEP, they would also be weaker interventions.

X. SUMMARY AND CONCLUSIONS

STEP is a five-site demonstration program that offers two summers of remediation, life skills instruction and work experience to a randomly assigned group of economically and academically disadvantaged 14- and 15-year-olds. This report documents the operation and the impacts of the program as of the end of Summer 1987. It describes the implementation of STEP in its third full summer of operation, when Cohort III youth were served for the first time and returning members of Cohort II received a second (and final) summer of STEP services. For Cohort III, we examine program impacts on educational and fertility-related outcomes observable by the end of the first summer. For Cohort II, we examine impacts discernible during the school year 1986-87, just before their second summer of participation, and impacts that are observable after their second summer. For Cohort I, we examine outcomes observed at the end of school year 1986-87, the first following completion of STEP, and also look at outcomes measured during follow-up interviews conducted one year following program completion. The cost of the 15-month program is also estimated. This chapter summarizes the report and discusses the implications of the findings for policy and program development.

STEP IMPLEMENTATION DURING THE THIRD SUMMER

Site experience and familiarity with the STEP model and its operational requirements resulted in a smoother implementation of the program during the third summer of the demonstration. Improvements in the SYS component of the program were also implemented during the 1986-87 academic term.

The Summer Components

Successful implementation of STEP during the third summer of the demonstration was facilitated in large part by the high numbers of returning program staff and the routinization of many institutional arrangements. These staff were familiar with the various STEP curricula, which were virtually unchanged between the second and third summers, and were supportive of the goals of the program. Each of the agencies knew their responsibilities and there was a more uniform implementation of the demonstration across the five sites. For the first time, all sites offered remediation during the morning hours only; previously, Seattle had employed a schedule that included both morning and afternoon remediation sessions. A reduction in remediation subsites from five to two in San Diego improved uniformity of local implementation, though the addition of a fifth subsite in Boston may have decreased uniformity in that site.

Improvements made to the **remediation** component in the second summer of the demonstration--lead teachers, silent sustained reading, improved teacher training, and increased technical assistance--were again employed during the third summer. Across sites, teachers continued to assess the instructional approach positively. Students' displayed high levels of attentiveness in remediation sessions and attended classroom activities an average of 85 percent of available time.

The **Life Skills and Opportunities** component of STEP continued to receive enthusiastic reception on the part of both teachers and students. The first volume, used for first-year participants, was highly rated. The second-summer volume, though well received, was thought to require some revision, mostly in the sessions that focus on careers, health and nutrition, and substance abuse.

The **work experience** component of STEP was also well-implemented during the demonstration's third year. Employers were familiar with the STEP program and were no longer resistant to the idea of hiring younger youth, including STEP treatments, who are available only part time. Most worksites were carryovers from previous years; still, upgraded job assignments were secured for many returning Cohort II youth. Differences in the nature of job assignments between treatment and control youth, however, were found in all sites except Fresno.

The School-Year Component

Treatment youth from the second cohort of STEP were eligible to receive support services during the 1986-87 school year. This was the second opportunity to implement the school-year support component as part of the demonstration. All sites operated an SYS component during this second term, with the San Diego program doing so for the first time. Improvements put in place for SY 1986-87 led to more uniformity in the level of contact with each youth across sites, and the establishment of SYS coordinators and advocates worked well. Implementing SYS continued to be a challenge, however--particularly in terms of getting youth to take advantage of the SYS services. Tutoring, a service identified by sites as critical for these youth, was difficult to provide because of a series of logistical problems in sites, including transportation barriers, scheduling problems and the availability of tutors. Youth also tend not to admit needing the tutoring services and are prone to missing appointments.

PARTICIPANT CHARACTERISTICS

Like the first two cohorts of STEP youth, Cohort III youth are typical of youth in need of an intervention like STEP. Primarily, they are at risk of dropping out of school and of becoming adolescent parents. As a group, these youth, most of whom are enrolled in the eighth and ninth grades, performed well below their grade levels on the Metropolitan Achievement Test, averaging a 6.0 level in reading and 7.0 level in math at baseline. Their school records revealed sporadic attendance patterns and many had been held back in one or more grades. Their economic status (all are JTPA-eligible) and the high percentage who were already sexually experienced (44%) increased the likelihood that these youth would drop out of school before graduation.

Cohort II treatment youth in all sites and control youth in San Diego and Seattle were invited to return for a second summer of STEP during 1987. Some significant differences were found between the treatment youth who returned for a second summer and those who chose not to return. Returning youth were older, more likely to be from an Asian or black racial/ethnic background, had higher baseline test scores, came from larger families, had less pre-STEP work experience and were less likely to have had a pre-STEP course in sex education than were youth who chose not to return.

Fewer differences were found between the returning controls in San Diego and Seattle and non-returning controls in those sites. The two groups were very similar, with the exception that control returnees in San Diego tended to have higher baseline skill levels, and less pre-STEP work experience and sexual experience than non-returnees in that site.

SUMMER IMPACTS

Research indicates that STEP had large, substantive effects on the third cohort's basic skills competencies and knowledge of contraception during the first summer of participation. Cohort III treatment youth experienced significant learning gains in both reading and math, and significantly increased their knowledge of contraception.

Control group learning losses, large in previous years of the demonstration, were somewhat smaller for Cohort III. The losses experienced by these educationally and economically disadvantaged youth averaged 0.2 of a grade equivalent in reading and 0.3 of a grade equivalent in math.

STEP's net effects on the basic skills of Cohort III participants during their first summer of participation were slightly smaller than those seen for the second cohort, but were greater than those seen for the first cohort. The net effects of participation were equivalent to 0.5 of a grade in reading and 0.6 of a grade in math. Not only were the losses experienced by treatment youth stemmed in both subjects, but small gains were produced. These results and the strong results seen for Cohort II seem to confirm that the improved design and implementation of the STEP curriculum in the second year have led to greater program impacts.

The effects of one summer of STEP on Cohort III's basic skills were almost as widespread as those seen in 1986 for Cohort II. Math scores for treatment youth of both genders, from all racial/ethnic groups except Asians, and from all demonstration sites except Fresno improved over the summer. Furthermore, males and females, blacks, "whites and others," and participants in Boston, San Diego and Seattle all significantly improved their reading performance as a result of STEP participation.

While the first-summer learning effects for Cohort III were more modest than those seen for Cohort II, they were still quite strong and indicate that the program experienced by the second and third cohorts is more likely to lead to the desired longer-term outcomes of school retention and graduation than that experienced by Cohort I, who showed smaller first-summer effects.

STEP's effects on Cohort III youth's knowledge of contraception were also quite strong. The impacts on two scales of knowledge of contraceptives and their availability were similar to those observed for Cohort II; treatments' mean baseline level of knowledge increased by more than 50 percent, while knowledge among controls did not change. These effects were found for both genders, all racial/ethnic groups and across all demonstration sites.

A priori, we did not expect to see effects on reported sexual behavior over the six-week period, since the curriculum sessions that most directly relate to sexual behavior, such as role-playing on how to say no, instruction on physiology and contraception and a visit to the health clinic are not delivered until mid- or late summer. Our expectations were confirmed; during the first summer, STEP did not appear to affect sexual behavior for Cohort III youth. At the end of the summer, sexually active treatment youth in Cohort III were only 7.3 percent more likely than controls to report having used contraception during their most recent intercourse, a marked contrast to the strong results seen in this regard for Cohort II.

SCHOOL-YEAR IMPACTS FOR COHORT II

We examined the effect of one summer's participation in STEP on Cohort II's subsequent school-year behavior and performance, paying particular attention to attendance, standardized test scores, credits earned, grade promotion and dropout behavior. For the entire treatment sample, no school-year outcome was significantly affected by the first-summer and SYS experience. However, the first summer of STEP was effective for particular subgroups of Cohort II youth. For instance, STEP participation improved attendance for Hispanic youth and youth in Boston.

Better school performance also appears to be related to contact with SYS advocates. Cohort II treatments who had the most contact with SYS advocates earned more credits toward graduation and were more likely to be promoted than controls and treatment youth with less SYS contact. We cannot, however, determine the direction of causality in this relationship. Since three of the five sites generally contacted youth at school, it could be that youth who performed better were simply easier to contact.

These interim school-year impacts were not as strong as we had expected they might be, given the strong first-summer impacts observed for Cohort II. Perhaps the lack of interim school-year impacts signals the importance of participation in the full 15-month program--both summers plus SYS activities. Our first analysis of postprogram results for Cohort I, presented in Chapter VIII, provides some evidence that two summers may produce school-year impacts. Before reviewing Cohort I's postprogram impacts, however, we summarize the findings for Cohort II's second summer.

SECOND-SUMMER RESULTS FOR COHORT II

Since second-summer data are not available for most control youth in Cohort II, we describe outcomes rather than impacts when considering what happened to Cohort II during the second summer of STEP. Basic skills and questionnaire data for a control group in the second summer were available only in San Diego and Seattle. Therefore, net impacts on academic outcomes for the second summer and 15-months can only be derived for those two sites. We did, however, obtain information on fertility-related outcomes for Cohort II treatments and controls from all five sites through follow-up interviews that were conducted shortly after the conclusion of the program. These data serve as proxies for end-of-program outcomes and are used to estimate net impacts of STEP participation across all sites.

During the second summer, Cohort II treatment youth who returned to STEP experienced no significant changes in either reading or math. Over the 15-month program period, however, these youth experienced gains in both reading (0.3 of a grade equivalent) and math (1.0 grade equivalent).

Analysis of the San Diego and Seattle samples suggests that STEP successfully stemmed summer learning losses in those sites. In spite of nearly equivalent first-summer losses and larger school-year gains, control youth in these sites lost significantly more during the second summer than treatment youth in both reading and math. The net impact on test scores of participation in a second summer of STEP in these two sites was equivalent to 0.7 of a grade in reading and 0.8 of a grade in math.

Cohort II youth continued to improve their knowledge of contraception during their second summer of participation, increasing this measure by approximately one-third over the first summer average. Returning treatments improved from being able to correctly answer an average of five of 12 contraceptive knowledge questions at baseline to an average of almost nine correct by the end of the 15 months. Similarly, their knowledge of the availability of contraception improved from only three correct responses on eight items to almost six correct by the end of the program.

The San Diego and Seattle data indicate that this increase in knowledge can be attributed to STEP participation. By the conclusion of STEP, treatment youth in the two sites correctly responded to significantly more items on both scales than control youth. Analysis of data obtained through follow-up interviews shortly after the program ended provides further evidence of STEP's impact on knowledge of contraception. Results indicate that participation in STEP significantly improved knowledge of contraception both absolutely and relative to controls among treatment youth of both genders, from all racial/ethnic groups and across all five sites.

Analysis of the follow-up interview data indicates that STEP participation has had no short-term impact on reported sexual activity. Among males, however, treatment youth reported an increase in use of contraception. Treatment males were 16.9 percent more likely to have used contraception during their most recent intercourse than control males.

POSTPROGRAM IMPACTS FOR COHORT I

Two types of postprogram impacts were observed for Cohort I youth--education-related outcomes for their first postprogram school year (1986-87), and academic, fertility-related and employment outcomes measured one year beyond program completion. The first impacts were determined by analysis of school records data for SY 1986-87; the second were estimated using data collected through follow-up interviews conducted during Fall 1987.

The analysis of school records indicates some modest postprogram benefits for Cohort I participants. Treatment youth, particularly blacks, scored significantly higher on standardized math tests administered in Spring 1987. In Boston and Seattle, the only sites where promotion data are available, treatment youth were more likely to be promoted at the conclusion of SY 1986-87.

By the end of SY 1986-87, fewer treatment youth than controls had dropped out of school, though the difference was not significant.

For females and Hispanic youth, however, STEP participation significantly reduced the dropout rate at the conclusion of the school year. By the time of follow-up interviews, treatment and control females had similar dropout rates, but Hispanic control youth continued to exhibit higher dropout rates than Hispanic treatments.

A year following completion of STEP, Cohort I control youth exhibited no increase in their knowledge of contraception, while treatment youth retained a substantial portion of the knowledge they gained from STEP. Treatment and control youth were equally likely to report being sexually experienced and recently sexually active. Treatment youth were slightly more likely to report contraceptive use, but the impact was not statistically significant.

There was no overall program impact on the reported incidence of pregnancy among females or responsibility for pregnancy among males. There were, however, differences in reported pregnancy outcomes for those pregnancies that did not result in a live birth among females. Treatment females reported more abortions and fewer miscarriages than controls, while controls were more likely not to report the outcome of a pregnancy.

STEP participation also had no impact on either employment or earnings during the 1987 summer. Approximately 73 percent of both treatment and control youth were employed between July and September 1987. Controlling for weeks on the job, quarterly earnings and average wages were similar for treatments and controls. Since most Cohort I youth are still attending school full time, however, it may be too early for any real impact on employment and earnings to be evident.

COSTS

The social costs of enrolling a youth in the 15-month STEP program are derived from analysis of a synthetic (hypothetical) cohort. The estimated social cost per enrollee of providing 15 months of STEP, beyond the cost of providing one summer of work experience alone, was $1,685.[104] Portland, San Diego and Seattle had total social costs per enrollee of between $1,700 and $1,900. Boston and Fresno had lower 15-month per enrollee costs--$1,389 and $1,140 respectively. Our best estimate of the net cost of operating a mature STEP intervention that includes an active SYS program for sites enrolling approximately 150 new participants per year is the demonstration average of $1,685 per enrollee.

These costs are social costs and do not necessarily reflect the actual site costs of running the program. The average out-of-pocket costs were $1,947 per enrollee, of which $666 represented second-summer wages paid to the youth. Out-of-pocket costs are higher than social costs because they include wages. The general pattern of out-of-pocket costs across sites mirrors the social cost estimates; Boston and Fresno had lower out-of-pocket costs than the other three sites.

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[104] The average cost of providing only one summer of remediation and LSO instruction is $487 per enrollee.

IMPLICATIONS

The STEP experience and research to date provide some important insight into the need for and the viability and usefulness of summer-based strategies to improve basic skills and keep youth in school.

Although there is still a great deal to be learned about the summer learning loss phenomenon, such as whether it is sensitive to the type of test administered, and if and how losses can be retrieved during successive school years, available data indicate a need to reassess the educational importance of the summer period.

Such a reassessment should focus on the summer's emptiness of basic skills training rather than on its brevity as a limit on the opportunity for intervention. STEP, in fact, lasted only seven weeks, with remediation offered for about five half-days a week, yet it was sufficient to produce gains in both reading and math.

The nationwide summer jobs program provides an opportunity to test alternatives. Starting in Summer 1987, localities were required to assess the basic skills of the 650,000 youth enrolled in the program and to provide remedial assistance to participants with weak basic skills.

The STEP experience in remediation, particularly the differences in approach and test results between Summer 1985 and the 1986 and 1987 summers, should offer some useful insight into the level of resources and educational structures that are necessary to produce short-term test results that show promise for long-term improvements in basic skills. Treatment youth participating in STEP have consistently outperformed control youth on end-of-summer standardized reading and math tests. What is less clear at this point is whether these strong in-program impacts will eventually translate into long-term benefits.

Neither of the first two cohorts demonstrated improved school-year behavior relative to controls following the first summer of STEP participation. Among Cohort I youth, however, we observed some modest improvements during the school year following completion of the full program. The improvement in math scores, particularly strong among black youth, and both the improved attendance and reduction in dropout behavior among Hispanic youth suggest STEP's potential for longer-term impacts on educational outcomes. These results also imply that one summer, even when combined with the stronger SYS experienced by Cohort II, is insufficient to produce improvements in school-related behavior.

STEP's operational experience and test results confirm both the feasibility and importance of extended educational programming for high-risk students. STEP differs from simple school-year extension, however, in that it incorporates an opportunity for low-income youth to work and earn a salary into a summer academic program. It also allows youth to discuss and learn about key life options and their implications. These additional elements may be crucial in providing the economic incentive and practical knowledge necessary for continued participation in regular schooling.

With regard to fertility-related impacts, we have consistently observed significant increases in contraceptive knowledge among treatment youth; however, that increased knowledge does not seem to translate into behavioral changes. Possibly, the lessons of the LSO curriculum need to be reinforced during SYS activities. Alternatively, the program may be effective in ways that current analyses have been unable to detect for some subgroups of participants, such as youth who were not yet sexually active when they entered the program. Although they are outside the scope of this report, further analyses of the LSO-related outcomes are planned in order to explore these possibilities.

We have not yet observed any impact on the incidence of pregnancy, and the results regarding contraceptive practices are inconclusive. However, we have not observed any negative effects of LSO participation. Contrary to what opponents of family planning services might fear, the increased knowledge of contraception exhibited by STEP treatment youth does not appear to have translated into increased sexual activity.

It is still early to observe long-term effects of STEP participation on either educational or fertility-related outcomes. This is even more true for employment outcomes. Even among Cohort I youth, the majority of whom were in the 10th and 11th grade during SY 1986-87, the dropout and pregnancy rates are still relatively low for both treatments and controls. As time goes by and both rates increase, we may begin to detect significant impacts of the program.

The majority of outcomes examined to date seem to be positive. Although we have yet to observe long-term postprogram results, the overall trend suggests that some of the modest effects we have seen may eventually produce significant impacts, at least for some participants. Given the lack of effective programs for disadvantaged youth, an eventual reduction of dropout rates among

Hispanic youth or improvement in the basic skill levels of black youth would constitute substantively important findings.

Finally, the STEP experience to date demonstrates the feasibility of getting public education and employment/training institutions to work together on providing innovative, multidimensional, effective programming for high-risk youth.

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APPENDIX A

PROGRAM ATTENDANCE ANALYSIS

Before analyzing program impacts, we examined the determinants of program attendance. This analysis considers what characteristics predict how much remediation and work the youth receive. Factors found to be good predictors of program attendance may enable us to adjust the program's target population in order to increase the number of youth who complete the program.

For treatment youth, program hours were divided between classroom hours (including remediation and life skills classes) and work hours. As discussed in Chapter IV, Cohort III treatment youth attended an average of 100 classroom hours. Multivariate analysis was used to investigate the influence of a number of variables on classroom hours attended. Results of this analysis, shown in Table A.1, indicate that there were no significant differences in classroom attendance by gender or race/ethnicity. Youth with better basic reading skills at baseline tended to have better attendance, but the best predictor of classroom attendance was the youth's reported attendance during the preceding school year. Youth who reported being absent fewer than 10 days during the 1986-87 school year had better remediation/life skills attendance during STEP. Thus, it appears that the most motivated youth (as indicated by school-year attendance) get more exposure to the STEP treatment.

The second outcome examined was total hours attended. Total hours consisted only of work hours for control youth, but included both classroom and work hours for treatment youth. As discussed in Chapter IV, Cohort III control youth attended an average of 197 total hours while treatment youth attended an average of 199 hours. The multivariate analysis indicates that treatment status did not significantly influence total program hours attended, but a number of other variables did. Males attended more hours than females; Asian and black youth attended more hours than "whites and others." Youth who reported having problems with English and those with higher baseline reading scores had above average program attendance, while those with poor school-year attendance also attended fewer STEP program hours. Again, the impact of school-year attendance on program attendance seems to indicate that youth who are motivated to attend school are also motivated to participate in STEP.

Table A.1

DETERMINANTS OF PROGRAM HOURS

Classroom Hours		Total Hours	
Explanatory Variable	Coefficient	Explanatory Variable	Coefficient
Constant	35.34**	Constant	83.74***
Gender (male = 1)	-1.95	Treatment	2.48
Asian[a]	4.20	Gender (male = 1)	21.29*
Black[a]	-1.54	Asian[a]	21.06***
Hispanic[a]	-4.76	Black[a]	7.82**
Family Size	0.38	Hispanic[a]	7.50
Birth Order	0.93	Family Size	0.14
Grade Previous Year	2.00	Birth Order	0.21
Repeated a Grade	3.09	Grade Previous Year	1.99
School Absences	-9.21***	Repeated a Grade	1.96
Expects Post-High School Education	1.89	School Absences	-15.48***
English Language Problem	2.33	Expects Post-High School Education	0.34
Baseline Reading	0.05***	English Language Problem	7.35**
Boston[b]	14.78***	Baseline Reading	0.09***
Fresno[b]	6.45*	Boston[b]	-23.77***
Portland[b]	-1.54	Fresno[b]	59.29***
San Diego[b]	8.65***	Portland[b]	-22.89***
		San Diego[b]	1.12
		Lambda[c]	49.17**
Sample Size	634		1,239

[a] Racial/ethnic subgroup impacts are relative to "whites and others," the excluded category.
[b] Site impacts are relative to Seattle, the excluded catetory.
[c] Sample selection correction factor.

***Indicates that the impact statistically differs from zero at the 0.01 level.
**Indicates that the impact statistically differs from zero at the 0.05 level.

APPENDIX B

TABLES REFERENCED IN TEXT

Tables B.1 - B.6 (Chapter V)
Participant Characteristics at Baseline (Returnees in San Diego and Seattle)

Tables B.7 - B.14 (Chapter VI)
First-Summer Impact on Test Scores, Contraceptive Knowledge, and Sexual Behavior

Tables B.8 - B.19 (Chapter VII)
School-Year Outcomes (By Site for Cohort II; Overall and By Subgroups for Cohorts I and II)

Tables B.20 - B.22 (Chapter VII)
Second-Summer and 15-Month Outcomes for San Diego and Seattle)

Table B.1

DEMOGRAPHIC AND ECONOMIC CHARACTERISTICS OF RETURNEES IN SAN DIEGO

	Controls		Treatments	
	Non-Returnees	Returnees	Non-Returnees	Returnees
Age				
≤ 14 Years Old	52%	41%	54%	51%
≥ 15 Years Old	48	59	46	49
Gender				
Male	52%	46%	54%	46%
Female	48	54	46	54
Race/Ethnicity				
Asian	26%	31%	17%***	38%
Black	42	38	48	42
Hispanic	24	21	30 **	15
White & Other	8	10	6	5
Percentage With Limited English Proficiency	20%	23%	13%	30%
Percentage From Female-Headed Households	53%	41%	50%	43%
Mean Household Size	5.68	5.65	5.39**	6.25
Percentage Ever Worked	37%*	22%	28%**	13%
Sample Size	41	105	46	107

***The difference between returnees and non-returnees on this variable is significant at the 0.01 level.

**The difference between returnees and non-returnees on this variable is significant at the 0.05 level.

*The difference between returnees and non-returnees on this variable is significant at the 0.10 level.

Table B.2

EDUCATIONAL ABILITY AND SCHOOL PARTICIPATION CHARACTERISTICS OF RETURNEES IN SAN DIEGO

	Controls		Treatments	
	Non-Returnees	Returnees	Non-Returnees	Returnees
Educational Ability				
Mean MAT Reading Score	717**	735	718	732
(grade equivalent)	(5.8)	(6.7)	(5.8)	(6.6)
Mean MAT Math Score	712*	734	708*	737
(grade equivalent)	(7.0)	(7.8)	(5.3)	(7.9)
Grade in School				
8th Grade or Below	54%	62%	48%	51%
9th Grade	36	29	37	44
10th Grade & Above	10	9	15	5
Grade Retention History				
Percentage Ever Repeating a Grade	28%	20%	18%	23%
Absenteeism				
Percentage Never Absent	10%*	22%	17%	21%
Percentage Absent 1-10 Days in SY 86-87	71	59	65	63
Percentage Absent 11-20 Days in SY 86-87	5	14	11	11
Percentage Absent More Than 20 Days in SY 86-87	14	14	6	5
Educational Expectations				
Percentage Planning to Pursue Post-High School Education	73%	70%	72%	74%
Sample Size	41	105	46	107

**The difference between returnees and non-returnees on this variable is significant at the 0.05 level.

*The difference between returnees and non-returnees on this variable is significant at the 0.10 level.

Table B.3

FERTILITY-RELATED CHARACTERISTICS OF RETURNEES IN SAN DIEGO

Characteristic	Controls		Treatments	
	Non-Returnees	Returnees	Non-Returnees	Returnees
Percentage With Prior Sexual Experience	60%***	38%	46%*	30%
Percentage of the Sexually Active Sample Who Were Sexually Active in the Past Two Months	57%	40%	45%	63%
Percentage of the Recently Active Sample Never Using Contraception During Past Two Months	32%	26%	23%	28%
Mean Age at Onset of Sexual Activity for the Sexually Active Sample	12.6	11.5	11.9	12.2
Mean Number Correct on Birth Control Availability Questions (eight-item scale)	3.32	3.30	3.15	3.06
Mean Number Correct on General Birth Control Knowledge Questions (12-item scale)	5.05	5.30	5.37	4.85
Percentage Who Have Taken a Prior Sex Education Course	90%	90%	84%	88%

***The difference between returnees and non-returnees on this variable is significant at the 0.01 level.

*The difference between returnees and non-returnees on this variable is significant at the 0.10 level.

Table B.4

DEMOGRAPHIC AND ECONOMIC CHARACTERISTICS OF RETURNEES IN SEATTLE

	Controls		Treatments	
	Non-Returnees	Returnees	Non-Returnees	Returnees
Age				
$\leq$ 14 Years Old	56%	53%	49%	50%
$\geq$ 15 Years Old	44	47	51	50
Gender				
Male	44%	56%	44%	57%
Female	56	44	56	43
Race/Ethnicity				
Asian	19%	26%	12%**	27%
Black	64	58	60	54
Hispanic	0	3	5	2
White & Other	17	13	23	17
Percentage With Limited English Proficiency	12%	22%	3%	12%
Percentage From Female-Headed Households	58%	51%	55%	52%
Mean Household Size	4.67	4.83	4.16***	5.05
Percentage Ever Worked	35%	27%	29%	30%
Sample Size	43	113	43	122

***The difference between returnees and non-returnees on this variable is significant at the 0.01 level.

*The difference between returnees and non-returnees on this variable is significant at the 0.10 level.

270

Table B.5

EDUCATIONAL ABILITY AND SCHOOL PARTICIPATION CHARACTERISTICS OF RETURNEES IN SEATTLE

	Controls		Treatments	
	Non-Returnees	Returnees	Non-Returnees	Returnees
Educational Ability				
Mean MAT Reading Score	722	715	731	724
(grade equivalent)	(6.0)	(5.7)	(6.5)	(6.2)
Mean MAT Math Score	694	687	683	695
(grade equivalent)	(6.5)	(6.3)	(6.2)	(6.5)
Grade in School				
8th Grade or Below	56%	51%	66%	50%
9th Grade	28	41	28	36
10th Grade & Above	16	8	6	14
Grade Retention History				
Percentage Ever Repeating a Grade	42%	32%	52%***	25%
Absenteeism				
Percentage Never Absent	13%	15%	3%	11%
Percentage Absent 1-10 Days in SY 86-87	65	67	52	68
Percentage Absent 11-20 Days in SY 86-87	5	12	26	11
Percentage Absent More Than 20 Days in SY 86-87	17*	6	19	10
Educational Expectations				
Percent Planning to Pursue Post-High School Education	83%	72%	74%	74%
Sample Size	43	113	43	122

***The difference between returnees and non-returnees on this variable is significant at the 0.01 level.

Table B.6

FERTILITY-RELATED CHARACTERISTICS OF RETURNEES IN SEATTLE

Characteristic	Controls		Treatments	
	Non-Returnees	Returnees	Non-Returnees	Returnees
Percentage With Prior Sexual Experience	44%	48%	48%	43%
Percentage of the Sexually Active Sample Who Were Sexually Active in the Past Two Months	42%	48%	53%	50%
Percentage of the Recently Active Sample Never Using Contraception During Past Two Months	29%	21%	0	9%
Mean Age at Onset of Sexual Activity for the Sexually Active Sample	12.5	11.9	12.0	12.3
Mean Number Correct on Birth Control Availability Questions (8-item scale)	3.07	2.89	3.22	3.16
Mean Number Correct on General Birth Control Knowledge Questions (12-item scale)	5.02	4.73	5.19	5.21
Percentage Who Have Taken a Prior Sex Education Course	61%	67%	83%***	62%

***The difference between returnees and non-returnees on this variable is significant at the 0.01 level.

Table B.7

PRETEST TO POSTTEST CHANGES IN MEAN READING AND MATH SCORES BY GENDER AND RACE/ETHNICITY FOR AGGREGATE SAMPLE OF COHORTS II AND III

Group (Sample Size)	Reading Scores: Change from Pre- to Posttest	Reading Scores: Treatment/Control Difference	Math Scores: Change from Pre- to Posttest	Math Scores: Treatment/Control Differences
Total Sample				
Treatment (1,272)	-1.18	11.33***	6.67***	20.40***
Control (1,239)	-12.51***		-13.73***	
Males				
Treatment (578)	-5.50**	14.24***	2.84	24.10***
Control (629)	-19.74***		-21.26***	
Females				
Treatment (694)	2.42	7.47***	9.86***	15.82***
Control (610)	-5.05***		-5.96***	
Asians				
Treatment (248)	10.65***	5.80*	12.67***	11.67***
Control (216)	4.85**		1.00	
Blacks				
Treatment (608)	-5.26***	12.85***	5.13**	22.12***
Control (621)	-18.11***		-16.99***	
Hispanics				
Treatment (235)	1.97	8.19*	5.10	19.10***
Control (218)	-6.22*		-14.00***	
Whites & Others				
Treatment (181)	-7.75*	13.68**	5.65	25.31***
Control (184)	-21.43***		-19.66***	

***Indicates that the change statistically differs from zero at the 0.01 level.
**Indicates that the change statistically differs from zero at the 0.05 level.
*Indicates that the change statistically differs from zero at the 0.10 level.

Table B.8

PRETEST TO POSTTEST CHANGES IN MEAN READING AND MATH SCORES BY SITE FOR AGGREGATE SAMPLE OF COHORTS II AND III

Group (Sample Size)		Reading Scores		Math Scores	
		Change from Pre- to Posttest	Treatment/Control Difference	Change from Pre- to Posttest	Treatment/Control Differences
Total Sample					
Treatment	(1,272)	-1.18	11.33***	6.67***	20.40***
Control	(1,239)	-12.51***		-13.73***	
Boston					
Treatment	(255)	1.02	15.41***	11.21***	23.27***
Control	(247)	-14.39***		-12.06***	
Fresno					
Treatment	(252)	-2.69	4.82	1.85	17.92***
Control	(254)	-7.51***		-16.07***	
Portland					
Treatment	(262)	-10.84***	6.30	6.21*	17.06***
Control	(253)	17.14**		-10.85***	
San Diego					
Treatment	(262)	11.39***	13.68***	11.07***	14.44***
Control	(252)	-2.29		-3.37	
Seattle					
Treatment	(241)	-5.10	16.88***	2.63	29.89***
Control	(233)	-21.98***		-27.26***	

***Indicates that the change statistically differs from zero at the 0.01 level.
**Indicates that the change statistically differs from zero at the 0.05 level.
*Indicates that the change statistically differs from zero at the 0.10 level.

Table B.9

PRETEST TO POSTTEST CHANGES IN MEAN READING AND MATH SCORES BY GENDER AND RACE/ETHNICITY FOR AGGREGATE SAMPLE OF COHORTS I, II AND III

Group (Sample Size)	Reading Scores		Math Scores	
	Change from Pre- to Posttest	Treatment/Control Difference	Change from Pre- to Posttest	Treatment/Control Differences
Total Sample				
Treatment (1859)	-3.03***	10.18***	4.66***	19.11***
Control (1831)	-13.21***		-14.45***	
Males				
Treatment (877)	-7.74***	11.44***	-0.60	22.57***
Control (937)	-19.18***		-23.17***	
Females				
Treatment (982)	1.18	8.14***	9.36***	14.67***
Control (894)	-6.96***		-5.31***	
Asians				
Treatment (337)	10.06***	7.47***	12.45***	11.41**
Control (304)	2.59		1.04	
Blacks				
Treatment (966)	-7.32***	9.33***	3.14	19.95***
Control (954)	-16.65***		-16.81***	
Hispanics				
Treatment (326)	-0.20	11.96***	0.98	21.09***
Control (309)	-12.16***		-20.11***	
Whites & Others				
Treatment (230)	-8.20**	12.03**	4.84	22.00***
Control (264)	-20.23***		-17.16***	

***Difference is significantly different from zero at the 0.01 level.
**Difference is significantly different from zero at the 0.05 level.
*Difference is significantly different from zero at the 0.10 level.

Table B.10

PRETEST TO POSTTEST CHANGES IN MEAN READING AND MATH SCORES BY SITE FOR AGGREGATE SAMPLE OF COHORTS I, II AND III

Group (Sample Size)	Reading Scores		Math Scores	
	Change from Pre- to Posttest	Treatment/Control Difference	Change from Pre- to Posttest	Treatment/Control Difference
Total Sample				
Treatment (1,859)	-3.03***	10.18***	4.66***	19.11***
Control (1,831)	-13.21***		-14.45***	
Boston				
Treatment (376)	-1.22	17.61***	8.99***	30.15***
Control (371)	-18.83***		-21.16	
Fresno				
Treatment (376)	-2.91	5.53*	-0.16	18.73***
Control (361)	-8.44***		-18.89***	
Portland				
Treatment (379)	-9.50***	3.66	5.36	12.55***
Control (373)	-13.16***		-7.19**	
San Diego				
Treatment (368)	4.63**	10.50***	5.58	11.47**
Control (373)	-5.87***		-5.89*	
Seattle				
Treatment (360)	-6.05**	13.94***	3.48	23.06***
Control (353)	-19.99***		-19.58***	

***Difference is significantly different from zero at the 0.01 level.
**Difference is significantly different from zero at the 0.05 level.

Table B.11

BASELINE TO ENDLINE CHANGES IN MEAN KNOWLEDGE OF CONTRACEPTION AND AVAILABILITY OF CONTRACEPTION FOR COHORT III OVERALL AND BY GENDER, RACE/ETHNICITY AND SITE

	Contraceptive Knowledge			Contraceptive Availability		
Group	Baseline	Endline	Change	Baseline	Endline	Change
Treatments	5.34	8.61	3.27***	3.33	5.64	2.31***
Controls	5.48	5.63	0.15*	3.48	3.54	0.06
Difference	-0.14	2.98***	3.12***	-0.15	2.10***	2.25***
Males						
Treatments	5.26	8.36	3.10***	3.11	5.26	2.15***
Controls	5.45	5.72	0.27***	3.23	3.33	0.10
Difference	-0.19	2.64***	2.83***	-0.13	1.93***	2.05***
Females						
Treatments	5.39	8.81	3.42***	3.51	5.94	2.43***
Controls	5.52	5.54	0.02	3.73	3.76	0.03
Difference	-0.13	3.27***	3.40***	-0.22	2.18***	2.40***
Asians						
Treatments	3.46	7.83	4.37***	2.06	5.52	3.46***
Controls	3.76	4.12	0.36*	2.83	2.73	-0.10
Difference	-0.30	3.71***	4.01***	-0.77***	2.79***	3.36***
Blacks						
Treatments	5.91	8.82	2.91***	3.69	5.63	1.94***
Controls	5.76	5.90	0.14	3.54	3.62	0.08
Difference	0.15	2.92***	2.77***	0.15	2.01***	1.86***
Hispanics						
Treatments	5.36	8.65	3.29***	3.49	5.74	2.25***
Controls	5.78	5.94	0.16	3.76	3.82	0.06
Difference	-0.42	2.71***	3.13***	-0.27	1.92***	2.19***

Table B.11 - (cont'd)

BASELINE TO ENDLINE CHANGES IN MEAN KNOWLEDGE OF CONTRACEPTION AND AVAILABILITY OF CONTRACEPTION FOR COHORT III OVERALL AND BY GENDER, RACE/ETHNICITY AND SITE

Group	Contraceptive Knowledge			Contraceptive Availability		
	Baseline	Endline	Change	Baseline	Endline	Change
Whites & Others						
Treatments	6.01	8.95	2.94***	3.71	5.69	1.98***
Controls	6.23	6.15	-0.08	3.67	3.94	0.27
Difference	-0.22	2.80***	3.02***	-0.04	1.75***	1.71***
Boston						
Treatments	5.34	7.61	2.27***	3.35	4.87	1.52***
Controls	5.42	5.39	-0.03	3.26	3.30	0.04
Difference	-0.08	2.22***	2.30***	0.09	1.57***	1.48***
Fresno						
Treatments	5.74	8.94	3.20***	3.53	5.69	2.16***
Controls	6.35	6.48	0.13	3.92	3.89	-0.03
Difference	-0.61**	2.46***	3.07***	-0.39	1.80***	2.19***
Portland						
Treatments	5.72	8.48	2.76***	3.43	5.37	1.94***
Controls	5.52	5.83	0.31*	3.37	3.57	0.20
Difference	0.20	2.65***	2.45***	0.06	1.80***	1.74***
San Diego						
Treatments	4.64	9.94	5.30***	3.02	7.06	4.04***
Controls	4.93	5.13	0.20	3.28	3.64	0.36**
Difference	-0.29	4.81***	5.10***	-0.26	3.42***	3.68***
Seattle						
Treatments	5.26	7.95	2.69***	3.36	5.09	1.73***
Controls	5.17	5.31	0.14	3.56	3.29	-0.27
Difference	0.19	2.64***	2.55***	-0.20	1.80***	2.00***

***Indicates that the change statistically differs from zero at the 0.01 level.
**Indicates that the change statistically differs from zero at the 0.05 level.
*Indicates that the change statistically differs from zero at the 0.10 level.

Table B.12

BASELINE TO ENDLINE CHANGES IN MEAN KNOWLEDGE OF CONTRACEPTION AND AVAILABILITY OF CONTRACEPTION FOR AGGREGATE SAMPLE OF COHORTS II AND III OVERALL AND BY GENDER, RACE/ETHNICITY AND SITE

	Contraceptive Knowledge			Contraceptive Availability		
Group	Baseline	Endline	Change	Baseline	Endline	Change
Treatments	5.26	8.24	2.98***	3.23	5.45	2.22***
Controls	5.31	5.45	0.15**	3.37	3.57	0.20***
Difference	-0.05	2.79***	2.84***	-0.14*	1.88***	2.02***
Males						
Treatments	5.33	8.15	2.82***	3.11	5.25	2.14***
Controls	5.31	5.48	0.17**	3.22	3.34	0.12
Difference	0.02	2.67***	2.65***	-0.11	1.91***	2.02***
Females						
Treatments	5.21	8.31	3.10***	3.32	5.61	2.29***
Controls	5.32	5.41	0.09	3.50	3.80	0.30***
Difference	-0.11	2.90***	3.01***	-0.18*	1.81***	1.99***
Asians						
Treatments	3.53	7.74	4.21***	2.19	5.58	3.39***
Controls	3.62	3.83	0.21	2.47	2.53	0.06
Difference	-0.09	3.91***	4.00***	-0.28*	3.05***	3.33***
Blacks						
Treatments	5.79	8.45	2.66***	3.51	5.40	1.89***
Controls	5.67	5.78	0.11	3.55	3.70	0.15**
Difference	0.12	2.67***	2.55***	-0.04	1.70***	1.74***
Hispanics						
Treatments	5.30	8.14	2.84***	3.38	5.56	2.18***
Controls	5.55	5.75	0.20	3.59	3.90	0.31***
Difference	-0.25	2.39***	2.64***	-0.21	1.66***	1.87***

Table B.12 - (cont'd)

BASELINE TO ENDLINE CHANGES IN MEAN KNOWLEDGE OF CONTRACEPTION AND AVAILABILITY OF CONTRACEPTION FOR AGGREGATE SAMPLE OF COHORTS II AND III OVERALL AND BY GENDER, RACE/ETHNICITY AND SITE

	Contraceptive Knowledge			Contraceptive Availability		
	Baseline	Endline	Change	Baseline	Endline	Change
Whites & Others						
Treatments	5.83	8.35	2.52***	3.49	5.29	1.80***
Controls	5.85	5.92	0.07	3.58	3.97	0.39***
Difference	-0.02	2.43***	2.45***	-0.09	1.32	1.41***
Boston						
Treatments	5.06	6.94	1.88***	3.14	4.56	1.42***
Controls	5.33	5.44	0.11	3.31	3.61	0.30***
Difference	-0.27	1.50***	1.77***	-0.17	0.95***	1.12***
Fresno						
Treatments	5.54	8.40	2.86***	3.26	5.35	2.09***
Controls	5.73	5.89	0.16	3.53	3.80	0.27***
Difference	-0.19	2.51***	2.70***	-0.27	1.55***	1.82***
Portland						
Treatments	5.56	8.33	2.77***	3.39	5.36	1.97***
Controls	5.36	5.59	0.23*	3.39	3.52	0.13
Difference	0.20	2.74***	2.54***	0.00	1.84***	1.84***
San Diego						
Treatments	4.80	9.80	5.00***	3.04	6.79	3.75***
Controls	5.09	5.11	0.02	3.25	3.50	0.25**
Difference	-0.29	4.69***	4.98***	-0.21	3.29***	3.50***
Seattle						
Treatments	5.39	7.58	2.19***	3.34	5.06	1.72***
Controls	5.04	5.19	0.15	3.35	3.39	0.04
Difference	0.35	2.39***	2.04***	-0.01	1.67***	1.68***

***Indicates that the change statistically differs from zero at the 0.01 level.
**Indicates that the change statistically differs from zero at the 0.05 level.
*Indicates that the change statistically differs from zero at the 0.10 level.

Table B.13

NET IMPACT OF PARTICIPATION IN STEP ON BIRTH CONTROL KNOWLEDGE VARIABLES FOR THE AGGREGATE SAMPLE OF COHORTS II AND III BY GENDER, RACE/ETHNICITY AND SITE

	Aggregate Sample		
Group	Contraceptive Knowledge[a]	Knowledge of the Availability of Contraception[b]	Knowledge of the Consequences of Teen Pregnancy[c]
Overall	2.87***	1.93***	0.25***
Gender	#		
Male	2.71***	1.95***	0.24***
Female	3.02***	1.90***	0.26***
Race/Ethnicity	###	###	
Asian	3.99***	3.12***	0.33***
Black	2.61***	1.67***	0.27***
Hispanic	2.61***	1.74***	0.17***
White & Other	2.65***	1.49***	0.19***
Site	###	###	###
Boston	1.73***	1.09***	0.11**
Fresno	2.68***	1.67***	0.15***
Portland	2.60***	1.74***	0.10*
San Diego	4.93***	3.42***	0.60***
Seattle	2.26***	1.60***	0.29***

NOTE: The sample size for this analysis is 2,463. Impacts for each subgroup are computed as the sum of the treatment coefficient plus each subgroup treatment interaction term coefficient. Significance is then determined by the appropriate F-test of the hypothesis that this sum equals zero.

[a] Mean number of correct responses on a 12-item scale.
[b] Mean number of correct responses on an eight-item scale.
[c] Mean number of correct responses on a two-item scale.

***Indicates that the impact differs statistically from zero at the 0.01 level.
**Indicates that the impact differs statistically from zero at the 0.05 level.
*Indicates that the impact differs statistically from zero at the 0.10 level.

###Indicates that the impact differs with respect to this characteristic at a 0.01 level of significance.
##Indicates that the impact differs with respect to this characteristic at a 0.05 level of significance.
#Indicates that the impact differs with respect to this characteristic at a 0.10 level of significance.

Table B.14

BASELINE TO ENDLINE CHANGES IN THE PERCENTAGE OF THE AGGREGATE SAMPLE OF COHORTS II AND III YOUTH WITH SEXUAL EXPERIENCE, HAVING SEX IN THE PREVIOUS TWO MONTHS, AND USING CONTRACEPTIVES AT LAST INTERCOURSE

	Sexual Experience[a]		Sexually Active[b]		Contraceptive Use[c]	
	Baseline	Change Over Summer	Baseline	Change Over Summer	Baseline	Change Over Summer
Treatments	41.6%	7.0***	53.8%	7.5***	39.0%	7.2***
Controls	46.6	6.6***	53.2	7.8***	35.0	2.6
Difference		0.4		-0.3		4.6
Males						
Treatment	58.7%	6.8***	55.1%	6.3	40.8%	7.4**
Control	64.1	8.8***	53.9	9.9	33.2	5.0*
Difference		-2.0		-3.6		2.4
Females						
Treatment	27.6%	7.1***	51.6%	9.4**	35.7%	7.0
Control	29.0	4.6***	51.6	3.2	39.0	-2.5
Difference		2.5*		6.2		9.5

[a] Percentage of youth reporting that they have ever had sex.

[b] Percentage of sexually experienced youth reporting that they have had sex in the previous two months.

[c] Percentage of sexually experienced youth reporting use of contraception the last time they had sex.

***Indicates that the change differs statistically from zero at the 0.01 level.
**Indicates that the change differs statistically from zero at the 0.05 level.
*Indicates that the change differs statistically from zero at the 0.10 level.

Table B.15

THE ESTIMATED IMPACTS OF STEP ON THE PERCENTAGE OF THE 1986-87 SCHOOL YEAR ABSENT BY SITE FOR COHORT II

Sample Site	Estimated Impact	Control Group Mean	Sample Size
Boston	-1.72%	18.26%	255
Fresno	-0.32	12.18	235
Portland	2.06	17.32	236
San Diego	n.a.	n.a.	n.a.
Seattle	1.62	11.95	233

Note: None of the estimates were statistically significant at the 0.10 level.

n.a. = not available.

Table B.16

THE ESTIMATED IMPACT OF STEP ON THE PERCENTAGE OF GRADUATION CREDITS EARNED DURING THE INTERVENING SCHOOL YEAR BY SITE FOR COHORT II

Sample Site	Estimated Impact (Percentage Points)	Sample Size
Boston	0.33	194
Fresno	1.44	210
Portland	-1.98	223
San Diego	0.02	214
Seattle	-0.98	213

Note: None of the estimates were statistically significant at the 0.10 level.

Table B.17

THE ESTIMATED IMPACT ON THE PROBABILITY OF PROMOTION DURING THE INTERVENING SCHOOL YEAR BY SITE FOR COHORT II

Sample Site	Estimated Impact (Percentage Points)	Percentage of Control Group Promoted	Sample Size
Boston	-2.53	81.25	255
Seattle	0.38	98.3	233

Note: Neither impact was statistically significant at the 0.10 level of confidence.

Table B.18

THE ESTIMATED IMPACTS OF STEP ON ABSENTEEISM AND TEST SCORES IN READING AND MATH DURING THE INTERVENING SCHOOL YEAR BY GENDER, RACE/ETHNICITY AND SITE FOR COHORTS I AND II

Group	Percentage of the Year Absent[a]	Reading Scores in NCEs[b]	Math Scores in NCEs[b]
Overall	-0.23	-0.33	0.63
Gender			
Male	-0.33	-1.33	0.79
Female	-0.13	0.63	0.48
Race/Ethnicity			
Asian	0.97	0.96	0.58
Black	-0.45	-0.07	1.07
Hispanic	-2.38*	-2.43*	0.05
White & Other	2.06	0.30	-0.33
Sites			
Boston	-1.94*	-0.60	0.86
Fresno	0.38	-0.19	1.25
Portland	0.31	n.a.	n.a.
San Diego	n.a.	-1.35	0.60
Seattle	0.64	1.07	-0.33
Sample Size	1,794	1,592	1,603

Note: We tested to see whether the impacts varied by gender, race/ethnicity and site. None of the subgroup impacts differed from another at a 0.10 level.

[a] San Diego did not provide absenteeism data.

[b] Portland test scoes were omitted because they were not comparable to those in other sites. Only 55 percent of the San Diego youth had test data.

*Indicates that the impact is statistically different from zero at the 0.10 level.

Table B.19

THE ESTIMATED IMPACTS OF STEP ON CREDITS EARNED DURING THE INTERVENING SCHOOL YEAR BY GENDER, RACE/ETHNICITY AND SITE FOR COHORTS I AND II

Characteristic	Percentage of Graduation Credits Earned
Overall	-0.83
Gender	
Male	-1.37
Female	-0.53
Race/Ethnicity	
Asian	-0.26
Black	-1.01
Hispanic	-0.37
White & Other	-1.78
Sites	
Boston	1.02
Fresno	-1.24
Portland	-2.13*
San Diego	-1.71
Seattle	-0.11
Sample Size	2,000

Note: We tested to see whether the impact varied by gender, race/ethnicity and site. None of the subgroup impacts differed from each other at a 0.10 level.

*Indicates that the impact is statistically different from zero at the 0.10 level.

Table B.20

CHANGES IN MEAN READING AND MATH SCORES OVER 15 MONTHS FOR COHORT II TREATMENT AND CONTROL YOUTH IN SAN DIEGO AND SEATTLE

	San Diego			Seattle		
	Treatments	Controls	Difference	Treatments	Controls	Difference
Reading Scores						
1986 Pre-test	733.54	734.94	-1.10	724.95	719.89	5.06
First-Summer Change	2.69	-11.42***	14.11***	-13.66**	-28.29***	14.63*
School-Year Change	-1.26	13.82***	-15.08***	23.66***	44.98***	-21.32***
Second-Summer Change	13.37***	1.65	11.72*	-8.81	-39.42***	30.61***
15-Month Change	14.79***	4.05	10.74	1.19	-22.73***	23.92***
Math Scores						
1986 Pre-test	743.72	728.62	15.10	697.36	689.68	7.68
First-Summer Change	20.95***	7.45	13.50	4.19	-27.83***	32.02***
School-Year Change	18.77***	15.22	3.55	18.84***	49.81***	-30.97***
Second-Summer Change	0.26	5.04	-4.78	-16.83**	-38.51***	21.68
15-Month Change	39.99***	27.71***	12.28	6.20	-16.53	22.73
Sample Size	87	78				

***Indicates that the change is statistically different from zero at the 0.01 level.
**Indicates that the change is statistically different from zero at the 0.05 level.
*Indicates that the change is statistically different from zero at the 0.10 level.

Table B.21

CHANGES IN MEAN KNOWLEDGE OF CONTRACEPTIVE METHODS AND AVAILABILITY OVER 15 MONTHS FOR COHORT II TREATMENT AND CONTROL YOUTH IN SAN DIEGO AND SEATTLE

	San Diego			Seattle		
	Treatments	Controls	Difference	Treatments	Controls	Difference
Mean Knowledge of Contraceptive Methods[a]						
1986 Pretest	4.83	5.03	-0.20	5.32	4.75	0.57
First-Summer Change	5.10***	-0.01	5.11***	1.61***	0.11	1.50***
School-Year Change	-1.22***	0.64***	-1.86***	-0.15	0.99***	-1.14***
Second-Summer Change	1.17***	0.04	1.13***	1.31	-0.34***	1.65***
15-Month Change	5.04***	0.67***	4.37***	2.77***	0.76***	2.01***
Mean Knowledge of Contraceptive Availability[b]						
1986 Pretest	3.02	3.35	-0.33	3.12	2.96	0.16
First-Summer Change	3.72***	0.01	3.71***	1.66***	0.20	1.46***
School-Year Change	-0.91***	0.90***	-1.81***	-0.08	0.56***	-0.64**
Second-Summer Change	1.28***	-0.18	1.46***	0.63***	0.01	0.62***
15-Month Change	4.09***	0.73***	3.36***	2.22***	0.76***	1.46***
Mean Knowledge of the Consequences of Teen Pregnancy[c]						
1986 Pretest	0.89	1.03	-0.15	0.83	0.93	-0.10
First-Summer Change	0.55***	-0.09	0.64***	0.28***	-0.15*	0.43***
School-Year Change	0.33***	0.56***	-0.23**	0.43***	0.75***	-0.32***
Second-Summer Change	0.07	0.10	-0.03	0.05	-0.12*	0.17*
15-Month Change	0.94***	0.58***	0.36***	0.77***	0.48***	0.29***

[a] Mean number correct responses on a 12-item scale.
[b] Mean number correct responses on an eight-item scale.
[c] Mean number correct responses on a two-item scale.

***Indicates that the change is statistically different from zero at the 0.01 level.
**Indicates that the change is statistically different from zero at the 0.05 level.

Table B.22

BASELINE TO ENDLINE CHANGES IN MEAN HEALTH KNOWLEDGE AND MEAN DRUG KNOWLEDGE FOR COHORT II TREATMENT AND CONTROL YOUTH IN SAN DIEGO AND SEATTLE

	San Diego			Seattle		
	Treatments	Controls	Difference	Treatments	Controls	Difference
Mean Health Knowledge[a]						
Pretest	2.65	2.61	-0.04	2.53	2.60	-0.07
Posttest	2.80	2.87	-0.07	2.81	2.57	0.24*
Change	0.15	0.26***	-0.11	0.28***	-0.03	0.31*
Mean Drug Knowledge[b]						
Pretest	4.30	4.24	0.06	4.05	4.28	-0.23
Posttest	4.49	3.45	1.04***	4.10	3.78	0.32
Change	0.19	-0.79***	0.98***	0.05	-0.50***	0.55**

[a] Mean number of correct responses on a four-item battery.
[b] Mean number of correct responses on a eight-item battery.

***Indicates that the change is statistically different from zero at the 0.01 level.
**Indicates that the change is statistically different from zero at the 0.05 level.
*Indicates that the change is statistically different from zero at the 0.10 level.

APPENDIX C

CONTROL GROUP LOSSES

As discussed in Chapter I, research indicates that summer is a period when learning often stops or decays, particularly among disadvantaged youth. The performance of STEP control group youth on both reading and math tests generally support this earlier finding. Control youth in the first two STEP cohorts experienced substantial losses over the summer--losing an average of nearly a full grade in reading and approximately half a grade in math.

The extent of control group losses is important because it represents the extent of loss in critical basic skills at-risk youth experienced during the summer--a time when they were away from school or any other academic intervention. We assume that similar losses would have been experienced by treatment youth in the absence of STEP.

Because control group summer learning losses are substantively important, measuring losses without error or bias is crucial. Unmeasured factors related to the testing situation may affect treatment and control youth differently and artificially inflate or minimize the observed control group losses; thus, P/PV took steps to specifically counteract two potential sources of bias in the testing situation--differential motivation to perform well and differential environmental and test proctor effects.

To counteract differential motivation, P/PV developed an incentive scheme designed to motivate both treatment and control youth to do their best at both the pretest and the posttest.[1] Treatment and control youth were also tested together in four of the five sites to ensure that any biases introduced by test proctors and environmental conditions would influence them equally. We believe these precautions produced comparable testing environments and conditions because there were no significant differences in the pretest scores of Cohort III treatments and controls except in Fresno, where control youth were pretested separately and had significantly higher math scores than treatment youth.[2]

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[1] The details of this incentive scheme have been discussed extensively in previous reports. Briefly, any youth taking the test receives one chance in a drawing for 15 $50 prizes awarded at each site. Youth also received an additional chance for each correct answer. Thus, youth can maximize their chances of winning by trying to correctly answer as many questions as possible. If these incentives have their predicted effects, any control group losses observed should be the result of true losses in measured academic skills.

As shown in Table C.1, overall losses on the math test from pretest to posttest among Cohort III controls were slightly less pronounced than those observed for controls in the first two cohorts. In reading, however, the small loss for Cohort III control youth (equivalent to 0.2 of a grade) contrasts sharply with the large losses recorded for control youth in Cohorts I and II. Not only were these losses smaller, control youth in two sites (Fresno and San Diego) actually showed reading gains over the summer.[3]

Perplexed by these findings, we investigated a number of possible explanations. First, we examined demographic and other baseline characteristics to determine whether Cohort III youth were somehow different than Cohort II youth.[4] This did not seem to have been the case. There was no increase in the percentage of Asians (who tend to experience academic gains over the summer) and no decrease in the percentage of black youth (who tend to experience large losses). There was no discernible difference between Cohort II and III in the number of youth from large families or who have been retained in a grade, both groups that tend to have lower test scores. The only baseline characteristic that was significantly different from Cohort II to Cohort III across all five sites was educational expectations--83 percent of Cohort III controls indicated that they expected to attain some education beyond high school, compared with only 72 percent of those from Cohort II. The generally higher educational expectations among Cohort III youth may reflect greater achievement motivation, which may have manifested itself in better test performance.

The smaller control group losses might also be partly explained by the type of jobs at which control youth worked. If youth

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[2] Although treatment and control youth were tested separately in Fresno, the same rooms and proctors were used for testing both groups, and testing occurred at the same time of day.

[3] Although the finding is not shown in the table, Asian control youth in Cohort III showed significant reading gains of almost 13 points (equivalent to 0.6 of a grade). And Hispanic control youth had smaller (0.2 of a grade equivalent), non-significant reading gains.

[4] We examined the composition of the two cohorts with respect to gender, race/ethnicity, age, family size, percentage from female-headed households, absenteeism, grade retention, educational expectations, problems with English, pretest scores and sexual experience.

Table C.1

MEAN CONTROL GROUP LOSSES IN READING AND MATH IN GRADE EQUIVALENTS BY SITE FOR THE PILOT SAMPLE, COHORT I, COHORT II AND COHORT III

Sample	Overall	Boston	Fresno	Portland	San Diego	Seattle	Pinellas County
Reading Scores							
Pilot[a]	n.a.	-1.1	n.a.	n.a.	n.a.	n.a.	-0.2
Cohort I	-0.7	-1.4	-0.6	-0.2	-0.6	-0.7	n.a.
Cohort II	-1.0	-0.5	-1.1	-1.4	-0.5	-1.6	n.a.
Cohort III	-0.2	-1.1	+0.4	-0.5	+0.3	-0.4	n.a.
Math Scores							
Pilot[a]	n.a.	-0.4	n.a.	n.a.	n.a.	n.a.	-0.4
Cohort I	-0.4	-1.0	-0.6	0.0	-0.4	-0.1	n.a.
Cohort II	-0.5	-0.2	-0.8	-0.5	+0.1	-1.0	n.a.
Cohort III	-0.3	-0.4	-0.3	-0.1	-0.3	-0.4	n.a.

n.a. = not available.

[a] Only two of the three pilot sites, Boston and Pinellas County, collected data for a randomly assigned control group. Mean test scores and changes were not estimated for the overall sample of control youth in the pilot study.

worked at jobs that required extensive use of the math and/or reading skills that are measured by the test, they would likely exhibit better performance on the MAT. There is some evidence, particularly in San Diego, that an appreciable number of control youth were able to obtain such jobs. For example, the Naval Base employed many control youth in jobs that generally required higher levels of skills than typical maintenance and recreation positions. Treatments were unable to accept these jobs because of their limited working hours. In addition, a significantly higher percentage of control youth than treatment youth in San Diego reported that they worked in clerical positions (31% vs. 20%), while significantly more treatments reported having worked in custodial/maintenance positions (43% vs. 32%).[5] In Boston, where control losses were greatest, the opposite pattern prevailed--51 percent of treatments and only 9 percent of controls worked at clerical jobs. Unfortunately, we do not have comparable information on Cohorts I or II; thus, we do not know whether these differential job assignments represent a change from previous summers.

As noted in Chapter V, differential attrition between treatments and controls from the randomization sample to the learning analysis sample introduces sample-selection bias. This bias can be controlled for statistically in the multivariate analysis, but not in the analysis of simple mean test scores. Control youth who took the pretest, but were lost to the analysis sample (either because they did not take the posttest or were missing information on other variables) had significantly lower average pretest scores (696.73 in reading and 687.84 in math) than those who were retained in the analysis sample (713.11 in reading and 704.63 in math). This suggests that control youth who were lost may be different from those with intact data. For example, they may have dropped out of SYETP, reflecting differences in motivation or persistence. Thus, their attrition from the sample may have attenuated the average learning loss experienced by all controls.

The general variation in control group losses across cohorts and across sites may also be an artifact of testing and measurement error. There are any number of unmeasured differences in the testing situations and/or characteristics of youth that can introduce "noise" into the data. These unmeasured factors may have contributed to the variation in the estimated control group losses, the most striking of which is an average loss of 1.6 grade levels in reading among Cohort II youth in Seattle, compared with a gain of 0.4 of a grade level among Cohort III youth in Fresno.

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[5] These job breakdowns were not matched in Fresno, however, where control youth also experienced gains in reading over the summer.

One explanation for the observed variance in control group losses is less evident because of its technical nature. Each of the reported mean control group losses (or gains) is an estimate of the "true" population mean based on a sample of youth from that larger population. These sample means will vary from sample to sample but will cluster around the true population mean. Although the sample means tend to cluster around the population mean, a particular sample may produce an estimate of the mean that is not close to the true value. Thus, the extreme estimates of control group losses and/or gains may simply be anomalies rather than accurate estimates of the true loss experienced by control youth.

APPENDIX D

TECHNICAL DATA TABLES

Table D.1

VARIABLE DESCRIPTIONS

Variable	Description	Type
ABS1-AN	AN (see Asian_AN) * ABSMUCH1 interaction term.	Dummy (0,1)
ABS1_CM	CM (see Asian_CM) * ABSMUCH1 interaction term.	Dummy (0,1)
ABSMUCH1	Participant was absent 11 or more days during the previous school year.	Dummy (0,1)
ADRPOUT6	Whether student has dropped out of school, measured at postprogram follow-up.	Dummy (0,1)
AGE1	1 if participant was 14 or under. 0 otherwise.	Dummy (0,1)
AKNWLDG1	Participant's knowledge of birth control methods (index based on number of correct answers to nine questions on the pretest).	Index
AKNWLDG2	As above (reported at postprogram follow-up).	Index
ASIAN	Self-explanatory	Dummy (0,1)
ASIAN_AN	ASIAN interacted with dummy (AN) where AN=1 if student is in the Fresno, Portland or Seattle site, 0 otherwise.	Index
ASIAN_CM	ASIAN interaced with dummy (CM) where CM=1 if student is in the Boston or San Diego site, 0 otherwise.	Index
ASIAN_T	ASIAN * TREAT interaction term.	
AVAILBL1	Participants' knowledge of the availability of birth control (index based on number of correct answers to eight availability questions on the pretest).	Index

Table D.1 - (cont'd)

VARIABLE DESCRIPTIONS

Variable	Description	Type
AVAILBL2	Participants' knowledge of the availability of birth control (index based on number of correct answers to eight availability questions on the posttest).	Index
BLACK	Self-explanatory.	Dummy (0,1)
BLACK_T	BLACK * TREAT interaction term.	Dummy (0,1)
BLCK_AN	AN (see Asian_AN) * BLACK interaction term.	Dummy (0,1)
BLCK_CM	CM (see Asian_CM) * BLACK interaction term.	Dummy (0,1)
BOS_T	BOSTON * TREAT interaction term.	Dummy (0,1)
BOSTON	Self-explanatory	Dummy (0,1)
BOST_TRT	BOSTON * TREAT interaction term.	Dummy (0,1)
BRTHORDR	Participants' birth order (e.g., would be three if participant was the third child born in the family).	Ordinal
BTODR-AN	AN (see Asian_AN) * BRTHORDR interaction term	Categorical
CNCONT11	Participant used birth control at last sexual contact, given that the participant had sex in the past two months. Reported at pretest.	Dummy (0,1)
CNCONTNR	Participant did not respond to contraceptive use question, given that participant had sex in the past two months. Non-response coded as "1", yes and no as "0". Reported at pretest	Dummy (0,1)

300

Table D.1 - (cont'd)

VARIABLE DESCRIPTIONS

Variable	Description	Type
CNCONTR2	Participant used birth control at last sexual contact, given that the participant had sex in the past two months. Reported at posttest.	Dummy (0,1)
CNCONTR5	Participant used birth control at last sexual contact. Reported at postprogram follow-up (Cohort 2).	Dummy (0,1)
CNCONTR6	Participant used birth control at last sexual contact. Reported at postprogram follow-up (Cohort 1).	Dummy (0,1)
DIEGO_T	San Diego * treatment interaction term.	Dummy ((0,1)
DROPOUT	Has participant dropped out of school? Restrictive definition reported by school district for SY 1986-87.	Dummy (0,1)
DROP2	Has participant dropped out of school? Less restrictive definition reported by school district for SY 1986-87.	Dummy (0,1)
EARNQ9	Quarterly earnings for July-September, 1987. Reported at postprogram follow-up.	Continuous
EJRD1-AN	AN (see Asian_AN) * ENJREAD1 interaction term.	Categorical
EJRD1_CM	CM (see Asian_CM) * ENJREAD1 interaction term.	Categorical
ENJMATH1	Measure of math enjoyment from 5 (very much) to 1 (not at all). Reported at pretest.	Categorical
ENJREAD1	Measure of reading enjoyment from 5 (very much) to 1 (not at all). Reported at pretest.	Categorical
EPHS1_AN	AN (see Asian_AN) * EPOSTHS1 interaction term.	Categorical

Table D.1 - (cont'd)

VARIABLE DESCRIPTIONS

Variable	Description	Type
EPHS1_CM	CM (see Asian_CM) * EPOSTHS1 interaction term.	Categorical
EPOSTHS1	Participant expects to obtain at least some post-high school education. Reported at pretest.	Dummy (0,1)
EVERPRG6	Has participant ever been pregnant? Reported at postprogram follow-up.	Dummy (0,1)
EVERSEX2	Has participant ever had sex? Reported at posttest.	Dummy (0,1)
EVERSX11	Has participant ever had sex? Reported at pretest.	Dummy (0,1)
EVERSXNR	Has participant ever had sex? Non-response coded as "1", yes and no as "0". Reported at pretest.	Dummy (0,1)
EVRPG11	Has participant ever been pregnant? Reported at pretest.	Dummy (0,1)
EVRPRGNR	Has participant ever been pregnant? Non-response coded as "1", yes and no coded as "0." Reported at pretest.	Dummy (0,1)
FAMSIZE1	Number of members in household including participant.	Ordinal
FATHCHD6	Has participant ever fathered a child? Reported at postprogram follow-up.	Dummy (0,1)
FTHCHD11	Has participant ever fathered a child? Reported at pretest.	Dummy (0,1)
FTHCHDNR	Has participant ever fathered a child? Non-response coded as "1", yes and no as "0." Reported at pretest.	Dummy (0,1)
FEMALE_T	FEMALE * TREAT interaction term	Dummy (0,1)

Table D.1 - (cont'd)

VARIABLE DESCRIPTIONS

Variable	Description	Type
FEMHDNR	Is participant's household headed by a female? Non-response coded as "1", yes and no coded as "0".	Dummy (0,1)
FEMHD11	Is participant's household headed by a female?	Dummy (0,1)
FMSZ1_AN	AN (see Asian_AN) * FAMSIZE1 interaction term.	Categorical
FMSZI_CM	CM (see Asian_CM) * FAMSIZE interaction term.	Categorical
FRESNO	Self-explanatory.	Dummy (0,1)
FRES_T	FRESNO * TREAT interaction term.	Dummy (0,1)
FRES_TRT	FRESNO * TREAT interaction term.	Dummy (0,1)
GRADE1	Grade in 1986-87 school year.	Categorical
GRD1_AN	AN (see Asian_AN) * GRADE1 interaction term.	Categorical
GRD1_CM	CM (see Asian_CM) * GRADE56 interaction term.	Categorical
HISPANIC	Self-explanatory.	Dummy (0,1)
HISP_AN	AN (see Asian_AN) * HISPANIC interaction term.	Dummy (0,1)
HISP_CM	CM (see Asian_CM) * HISPANIC interaction term.	Dummy (0,1)
HISP_T	HISPANIC * TREAT interaction term.	Dummy (0,1)
JBWKS	Of jobs held during the quarter July-September 1987, total number of weeks employed at longest job.	Continuous
JB_TREAT	JBWKS * TREAT interaction term.	Continuous

Table D.1 - (cont'd)

VARIABLE DESCRIPTIONS

Variable	Description	Type
JSPNSBL1	One if participant thinks both father and mother are responsible for birth control, "0" otherwise. Reported at pretest.	Dummy (0,1)
KNOWCON1	Two if participant knows consequences of teenage pregnancy to mother and the cost of raising a child to age 18. One if participant knows either. Zero if participant knows neither. Reported at pretest.	Index
KNOWCON2	As above at posttest.	Index
KNOWLDG1	Participant's knowledge of birth control methods (index based on number of correct answers to 12 questions on the pretest).	Index
KNOWLDG2	As above (reported on the posttest).	Index
LAMBDA	Sample selection correction factor for Cohort 3 learning analyses.	Continuous
LAMBDALS	Sample selection correction factor for Cohort 3 life skills related analyses.	Continuous
LANGPRB1	Participant speaks a language in addition to English and indicated problems speaking or understanding English.	Dummy (0,1)
LGPB1_AN	AN (see Asian_AN) * LANGPRB1 interaction term.	Dummy (0,1)
LGPB1_CM	CM (see Asian_CM) * LANGPRB1 interaction term.	Dummy (0,1)
MALE	Self-explanatory.	Dummy (0,1)
MALE_AN	AN (see Asian_AM) * MALE interaction term.	Dummy (0,1)

Table D.1 - (cont'd)

VARIABLE DESCRIPTIONS

Variable	Description	Type
MALE_CM	CM (see Asian_CM) * MALE interaction term.	Dummy (0,1)
MALE_T	MALE * TREAT interaction term.	Dummy (0,1)
MALE_TRT	MALE * TREAT interaction term.	Dummy (0,1)
MNCE	Normal curve equivalent to participant's math test score.	Continuous
OTHER_T	OTHER * TREAT interaction term.	Dummy (0,1)
PCTABS	Percent of time participant was absent during the school year.	Continuous
PCTCRED	Percent of possible credits earned by the participant.	Continuous
PORTLAND	Self-explanatory.	Dummy (0,1)
PORT_T	PORTLAND * TREAT interaction term.	Dummy (0,1)
PORT_TRT	PORTLAND * TREAT interaction term.	Dummy (0,1)
PROMOTED	Dummy for whether participant passed or failed.	Dummy (0,1)
RCNTSEXB	Has participant had sex in the last two months? Reported at posttest.	Dummy (0,1)
REPEAT1	Repeated a grade.	Dummy (0,1)
RNCE	Normal curve equivalent to participant's reading test score.	Continuous
RPT1_AN	AN (see Asian_AN) * REPEAT1 interaction term.	Dummy (0,1)
RPT1_CM	CM (see Asian-CM) * REPEAT1 interaction term.	Dummy (0,1)
SANDIEGO	Self-explanatory.	Dummy (0,1)
SAND_TRT	SANDIEGO * TREAT interaction term.	Dummy (0,1)

Table D.1 - (cont'd)

VARIABLE DESCRIPTIONS

Variable	Description	Type
SEATTL_T	SEATTLE * TREAT interaction term	Dummy (0,1)
SEXEDNR	Whether participant previously took a sex education course. Non-response coded as "1", yes and no as "0".	Dummy (0,1)
SEXED11	Whether participant previously took a sex education course.	Dummy (0,1)
STDMATH1	Math score (scaled) from MAT Survey Test intermediate level. Pretest (1st summer).	Continuous
STDMATH2	Math score (scaled) from MAT Survey Test intermediate level. Posttest (1st summer).	Continuous
STDMATH3	Math score (scaled) from MAT Survey Test intermediate level. Pretest (2nd summer).	Continuous
STDMATH4	Math score (scaled) from MAT Survey Test intermediate level. Posttest (2nd summer).	Continuous
STDREAD1	Reading score (scaled) for MAT Survey Test intermediate level. Pretest (1st summer).	Continuous
STDREAD2	Reading score (scaled) for MAT Survey Test intermediate level. Posttest (1st summer).	Continuous
STDREAD3	Reading score (scaled) for MAT Survey Test intermediate level. Pretest (2nd summer).	Continuous
STDREAD4	Reading score (scaled) for MAT Survey Test intermediate level. Posttest (2nd summer).	Continuous
STRDI_AN	AN (see Asian_AN) * STDREAD1 interaction term.	Continuous

Table D.1 - (cont'd)

VARIABLE DESCRIPTIONS

Variable	Description	Type
STRD1_CM	CM (see Asian_CM) * STDREAD1 interaction term.	Continuous
TODR_CM	CM (see Asian_CM) * BRTHORDR interaction term.	Categorical
TREAT	Treatment or control group.	Dummy (0,1)

Table D.2

REGRESSION ANALYSIS OF POSTTEST READING SCORES FOR COHORT III

DEP VARIABLE: STDREAD2
ANALYSIS OF VARIANCE

SOURCE	DF	SUM OF SQUARES	MEAN SQUARE	F VALUE	PROB>F
MODEL	19	2470167.52	130008.82	66.331	0.0001
ERROR	1231	2412748.58	1959.99073		
C TOTAL	1250	4882916.11			

ROOT MSE	44.27178	R-SQUARE	0.5059
DEP MEAN	720.546	ADJ R-SQ	0.4983
C.V.	6.1442		

PARAMETER ESTIMATES

VARIABLE	DF	PARAMETER ESTIMATE	STANDARD ERROR	T FOR H0: PARAMETER=0	PROB > |T|
INTERCEP	1	160.17284	6.00187217	6.160	0.0001
TREAT	1	9.33211809	2.81080281	3.320	0.0009
MALE	1	2.82157553	13.01870062	0.217	0.8285
ASIAN	1	15.73995452	6.12889135	2.568	0.0103
BLACK	1	-2.01893228	4.27683467	-0.472	0.6370
HISPANIC	1	4.08316656	5.12019539	0.797	0.4253
FAMSIZE1	1	-0.26573675	0.62710601	-0.424	0.6718
BRTHORDR	1	1.75967048	0.82248260	2.139	0.0326
GRADE1	1	4.67383563	2.01642624	2.318	0.0206
ABSMUCH1	1	-1.05702308	3.01320260	-0.351	0.7258
REPEAT1	1	-3.72226249	3.05220027	-1.220	0.2229
EPOSTHS1	1	2.75992586	3.47426381	0.794	0.4271
LANGPRB1	1	-2.92116289	4.62416652	-0.632	0.5277
ENJREAD1	1	2.88604413	1.26325216	2.285	0.0225
STDREAD1	1	0.66503842	0.02396122	27.755	0.0001
BOSTON	1	2.61965099	4.63513410	0.565	0.5721
FRESNO	1	18.73917996	4.50415081	4.160	0.0001
PORTLAND	1	10.14545408	4.30244821	2.358	0.0185
SANDIEGO	1	16.07819064	4.18002810	3.846	0.0001
LAMBDA	1	35.82669304	23.25449682	1.541	0.1237

Table D.3

REGRESSION ANALYSIS OF POSTTEST MATH SCORES FOR COHORT III

```
DEP VARIABLE: STDMATH2
ANALYSIS OF VARIANCE

SOURCE     DF       SUM OF SQUARES      MEAN SQUARE     F VALUE      PROB>F
MODEL      20         5551329.18        277566.46       76.902       0.0001
ERROR    1230         4439477.39        3609.33121
C TOTAL  1250         9990806.57

ROOT MSE              60.07771          R-SQUARE        0.5556
DEP MEAN              710.2958          ADJ R-SQ        0.5484
C.V.            8.458126

PARAMETER ESTIMATES

                       PARAMETER         STANDARD       T FOR HO:
VARIABLE     DF        ESTIMATE            ERROR        PARAMETER=0    PROB > |T|

INTERCEP     1       39.23176162      35.51999834        1.104          0.2696
TREAT        1       16.42511946       3.81329226        4.307          0.0001
MALE         1       13.68778837      17.64296599        0.776          0.4380
ASIAN        1       32.83410774       8.43159503        3.894          0.0001
BLACK        1       -1.17487597       5.82777112       -0.202          0.8403
HISPANIC     1        3.37249847       6.95310221        0.485          0.6277
FAMSIZE1     1       -1.50485967       0.85235262       -1.766          0.0777
BIRTHORDR    1        1.37552598       1.11583729        1.233          0.2179
GRADE1       1        6.56753809       2.74939375        2.389          0.0171
ABSMUCH1     1       -6.21607200       4.09051638       -1.520          0.1289
REPEAT1      1       -9.95421829       4.14502714       -2.401          0.0165
EPOSTHS1     1        3.58885247       4.76589975        0.753          0.4516
LANGPRB1     1        5.69683414       6.26247153        0.910          0.3632
ENJMATH1     1        4.88608184       1.57966686        3.093          0.0020
STDMATH1     1        0.55253987       0.02864003       19.293          0.0001
STDREAD1     1        0.21881951       0.03799210        5.760          0.0001
BOSTON       1       20.96983887       6.34982803        3.302          0.0010
FRESNO       1       28.78894942       6.25936402        4.599          0.0001
PORTLAND     1       25.85867956       5.87328113        4.403          0.0001
SANDIEGO     1       24.41253804       5.80368130        4.206          0.0001
LAMBDA       1       41.70153746      31.55048560        1.322          0.1865
```

Table D.4

REGRESSION ANALYSIS OF POSTTEST READING SCORES FOR COHORT III WITH SITE-TREATMENT INTERACTION TERMS

```
DEP VARIABLE: STDREAD2
ANALYSIS OF VARIANCE

SOURCE      DF      SUM OF SQUARES     MEAN SQUARE      F VALUE      PROB>F
MODEL       23        2476328.09       107666.44        54.894       0.0001
ERROR     1227        2406588.02       1961.35943
C TOTAL   1250        4882916.11

ROOT MSE              44.28724         R-SQUARE         0.5071
DEP MEAN              720.546          ADJ R-SQ         0.4979
C.V.                  6.146345

PARAMETER ESTIMATES

                          PARAMETER         STANDARD      T FOR H0:
VARIABLE       DF          ESTIMATE            ERROR    PARAMETER=0    PROB > |T|

INTERCEP        1         159.63662      26.10529965          6.115        0.0001
TREAT           1       10.71148202       6.01357166          1.781        0.0751
MALE            1        2.67285259      13.02597222          0.205        0.8375
ASIAN           1       15.96658022       6.13579632          2.602        0.0094
BLACK           1       -1.81378658       4.28124430         -0.424        0.6719
HISPANIC        1        4.33344171       5.12584265          0.845        0.3980
FAMSI           1       -0.27547532       0.62775211         -0.439        0.6609
BRTHC           1        1.62913834       0.82781054          1.968        0.0493
GRADE           1        4.63453188       2.01880893          2.296        0.0219
ABSMUCH1        1       -1.14914108       3.01707583         -0.381        0.7034
REPEAT1         1       -3.62677369       3.05418737         -1.187        0.2353
EPOSTHS1        1        2.62530352       3.48328529          0.754        0.4512
LANGPRB1        1       -2.90462863       4.63134906         -0.627        0.5307
ENJREAD1        1        2.82083505       1.26562427          2.229        0.0260
STDREAD1        1        0.66619075       0.02399882         27.759        0.0001
BOSTON          1        1.17902728       6.22486922          0.189        0.8498
FRESNO          1       22.26851225       6.10486756          3.648        0.0003
PORTLAND        1       13.10449535       5.90499273          2.219        0.0267
SANDIEGO        1       14.15777409       5.89674823          2.401        0.0165
BOST_TRT        1        2.78577771       8.19702165          0.340        0.7340
FRES_TRT        1       -6.99613641       8.11924797         -0.862        0.3890
PORT_TRT        1       -5.90502896       8.00415643         -0.738        0.4608
SAND_TRT        1        3.76251276       8.09930672          0.465        0.6423
LAMBDA          1       35.55549684      23.26439602          1.528        0.1267
```

Table D.5

REGRESSION ANALYSIS OF POSTTEST READING SCORES FOR COHORT III WITH RACIAL/ETHNIC-TREATMENT INTERACTION TERMS

```
DEP VARIABLE: STDREAD2
ANALYSIS OF VARIANCE

SOURCE      DF      SUM OF SQUARES      MEAN SQUARE      F VALUE      PROB>F
MODEL       22        2476156.85        112552.58        57.428       0.0001
ERROR     1228        2406759.26        1959.90167
C TOTAL   1250        4882916.11

ROOT MSE              44.27078          R-SQUARE         0.5071
DEP MEAN              720.546           ADJ R-SQ         0.4983
C.V.                  6.14406

PARAMETER ESTIMATES
                         PARAMETER          STANDARD        T FOR HO:
VARIABLE     DF          ESTIMATE            ERROR          PARAMETER=0     PROB > |T|

INTERCEP      1       157.17563          26.23734678          5.991          0.0001
TREAT         1        14.83888131        6.95635767          2.133          0.0331
MALE          1         2.57082686       13.02262535          0.197          0.8435
ASIAN         1        23.11303149        7.74184071          2.985          0.0029
BLACK         1        -0.20150873        5.80096638         -0.035          0.9723
HISPANIC      1         7.76539205        7.00729000          1.108          0.2680
FAMSIZE1      1        -0.25768660        0.62767361         -0.411          0.6815
BRTHORDR      1         1.76341999        0.82264395          2.144          0.0323
GRADE1        1         4.67352792        2.01726006          2.317          0.0207
ABSMUCH1      1        -1.08541586        3.01708321         -0.360          0.7191
REPEAT1       1        -3.82499983        3.05403752         -1.252          0.2106
EPOSTHS1      1         2.86678505        3.48912064          0.822          0.4114
LANGPRB1      1        -2.99983494        4.62613861         -0.648          0.5168
ENJREAD1      1         3.04040471        1.26687658          2.400          0.0165
STDREAD1      1         0.66491613        0.02397110         27.738          0.0001
BOSTON        1         2.50633772        4.63603458          0.541          0.5889
FRESNO        1        18.67297366        4.50438498          4.146          0.0001
PORTLAND      1        10.19371720        4.30246317          2.369          0.0180
SANDIEGO      1        16.08973687        4.17996409          3.849          0.0001
ASIAN_T       1       -13.95134837        9.04264717         -1.543          0.1231
BLACK_T       1        -3.52231981        7.72653965         -0.456          0.6486
HISP_T        1        -6.93429479        9.04391014         -0.767          0.4434
LAMBDA        1        35.28418107       23.25966006          1.517          0.1295
```

Table D.6

REGRESSION ANALYSIS OF POSTTEST READING SCORES FOR COHORT III WITH GENDER-TREATMENT INTERACTION TERMS

```
DEP VARIABLE: STDREAD2
ANALYSIS OF VARIANCE

SOURCE      DF       SUM OF SQUARES      MEAN SQUARE       F VALUE      PROB>F
MODEL       20         2470216.49        123510.82         62.966       0.0001
ERROR     1230         2412699.62        1961.54441
C TOTAL   1250         4882916.11

ROOT MSE               44.28933          R-SQUARE          0.5059
DEP MEAN               720.546           ADJ R-SQ          0.4979
C.V.                   6.146634

PARAMETER ESTIMATES
                         PARAMETER          STANDARD        T FOR H0:
VARIABLE    DF           ESTIMATE            ERROR          PARAMETER=0    PROB > |T|

INTERCEP     1          161.35480         27.06659002          5.961          0.0001
TREAT        1           8.80202926        4.37767175          2.011          0.0446
MALE         1           1.31267862       16.15030763          0.081          0.9352
ASIAN        1          15.59014733        6.20420507          2.513          0.0121
BLACK        1          -2.09489306        4.30545845         -0.487          0.6267
HISPANIC     1           4.14527812        5.13728867          0.807          0.4199
FAMSIZE1     1          -0.26331593        0.62754161         -0.420          0.6749
BRTHORDR     1           1.75879436        0.82282721          2.138          0.0328
GRADE1       1           4.69662664        2.02237661          2.322          0.0204
ABSMUCH1     1          -1.03497581        3.01762497         -0.343          0.7317
REPEAT1      1          -3.68757256        3.06129404         -1.205          0.2286
EPOSTHS1     1           2.75189985        3.47601178          0.792          0.4287
LANGPRB1     1          -2.90183892        4.62761558         -0.627          0.5307
ENJREAD1     1           2.88244540        1.26395801          2.280          0.0227
STDREAD1     1           0.66496532        0.02397518         27.736          0.0001
BOSTON       1           2.74241555        4.70162480          0.583          0.5598
FRESNO       1          18.72862786        4.50643063          4.156          0.0001
PORTLAND     1          10.23257813        4.33933491          2.358          0.0185
SANDIEGO     1          16.14833346        4.20518610          3.840          0.0001
MALE_TRT     1           0.89952443        5.69348574          0.158          0.8745
LAMBDA       1          33.91392278       26.22544623          1.293          0.1962
```

Table D.7

REGRESSION ANALYSIS OF POSTTEST MATH SCORES FOR COHORT III WITH SITE-TREATMENT INTERACTION TERMS

```
DEP VARIABLE: STDMATH2
ANALYSIS OF VARIANCE

SOURCE      DF     SUM OF SQUARES     MEAN SQUARE     F VALUE      PROB>F
MODEL       24       5562320.75       231763.36        64.162      0.0001
ERROR     1226       4428485.81       3612.14177
C TOTAL   1250       9990806.57

ROOT MSE              60.1011         R-SQUARE         0.5567
DEP MEAN             710.2958         ADJ R-SQ         0.5481
C.V.                 8.461418

PARAMETER ESTIMATES

                       PARAMETER          STANDARD      T FOR H0:
VARIABLE     DF        ESTIMATE           ERROR         PARAMETER=0     PROB > |T|

INTERCEP      1      41.03199275       35.67059844        1.150           0.2502
TREAT         1      14.08077102        8.15244354        1.727           0.0844
MALE          1      13.85181622       17.65256618        0.785           0.4328
ASIAN         1      33.30765645        8.44080530        3.946           0.0001
BLACK         1      -1.04583090        5.83383859       -0.179           0.8578
HISPANIC      1       3.72985893        6.96134884        0.536           0.5922
FAMSIZE1      1      -1.51292354        0.85332455       -1.773           0.0765
BRTHORDR      1       1.24378042        1.12327036        1.107           0.2684
GRADE1        1       6.60625038        2.75244142        2.400           0.0165
ABSMUCH1      1      -6.49292153        4.09621163       -1.585           0.1132
REPEAT1       1      -9.81222437        4.14777509       -2.366           0.0182
EPOSTHS1      1       3.68828438        4.77926755        0.772           0.4404
LANGPRB1      1       5.37392821        6.27217568        0.857           0.3917
ENJMATH1      1       4.86247825        1.58052790        3.076           0.0021
STDMATH1      1       0.55095234        0.02869844       19.198           0.0001
STDREAD1      1       0.21926009        0.03802661        5.766           0.0001
BOSTON        1      16.12658916        8.50427778        1.896           0.0582
FRESNO        1      32.81780772        8.42927313        3.893           0.0001
PORTLAND      1      23.48670997        8.02832389        2.925           0.0035
SANDIEGO      1      21.91096505        8.08603580        2.710           0.0068
BOST_TRT      1       9.65048507       11.12599809        0.867           0.3859
FRES_TRT      1      -7.92497585       11.02248200       -0.719           0.4723
PORT_TRT      1       4.81905699       10.85360450        0.444           0.6571
SAND_TRT      1       5.06277192       10.98568615        0.461           0.6450
LAMBDA        1      41.69275251       31.56455738        1.321           0.1868
```

Table D.8

REGRESSION ANALYSIS OF POSTTEST MATH SCORES FOR COHORT III WITH RACIAL/ETHNIC-TREATMENT INTERACTION TERMS

```
DEP VARIABLE: STDMATH2
ANALYSIS OF VARIANCE

SOURCE       DF      SUM OF SQUARES      MEAN SQUARE      F VALUE      PROB>F
MODEL        23        5580526.44        242631.58        67.503       0.0001
ERROR      1227        4410280.13        3594.36033
C TOTAL    1250        9990806.57

ROOT MSE               59.95298          R-SQUARE         0.5586
DEP MEAN               710.2958          ADJ R-SQ         0.5503
C.V.                   8.440566


PARAMETER ESTIMATES
                         PARAMETER           STANDARD        T FOR HO:
VARIABLE     DF          ESTIMATE            ERROR           PARAMETER=0    PROB > |T|

INTERCEP     1         29.72430874         35.73598898        0.832         0.4057
TREAT        1         36.60640460          9.42130972        3.885         0.0001
MALE         1         12.82543738         17.61221981        0.728         0.4666
ASIAN        1         51.05546766         10.61337557        4.810         0.0001
BLACK        1          8.85420638          7.87665359        1.124         0.2612
HISPANIC     1         15.64686141          9.48905939        1.649         0.0994
FAMSIZE1     1         -1.52583863          0.85126737       -1.792         0.0733
BRTHORDR     1          1.36440097          1.11379683        1.225         0.2208
GRADE1       1          6.43610655          2.74498056        2.345         0.0192
ABSMUCH1     1         -6.07975744          4.08753263       -1.487         0.1372
REPEAT1      1        -10.25942554          4.13881593       -2.479         0.0133
EPOSTHS1     1          4.45633803          4.77847531        0.933         0.3512
LANGPRB1     1          5.66215370          6.25300737        0.906         0.3654
ENJMATH1     1          4.73022126          1.57835365        2.997         0.0028
STDMATH1     1          0.55336604          0.02863555       19.324         0.0001
STDREAD1     1          0.21951725          0.03797484        5.781         0.0001
BOSTON       1         20.68337550          6.33747686        3.264         0.0011
FRESNO       1         28.65158960          6.24675011        4.587         0.0001
PORTLAND     1         25.99378393          5.86140468        4.435         0.0001
SANDIEGO     1         24.48541937          5.79204680        4.227         0.0001
ASIAN_T      1        -34.50078575         12.23897475       -2.819         0.0049
BLACK_T      1        -19.49437364         10.46654484       -1.863         0.0628
HISP_T       1        -23.26417754         12.24798508       -1.899         0.0577
LAMBDA       1         40.25801182         31.49346608        1.278         0.2014
```

Table D.9

REGRESSION ANALYSIS OF POSTTEST MATH SCORES FOR COHORT III WITH GENDER-TREATMENT INTERACTION TERMS

```
DEP VARIABLE: STDMATH2
ANALYSIS OF VARIANCE

SOURCE      DF      SUM OF SQUARES      MEAN SQUARE      F VALUE      PROB>F
MODEL       21        5551447.12        264354.62        73.184       0.0001
ERROR     1229        4439359.44        3612.17205
C TOTAL   1250        9990806.57

ROOT MSE              60.10135          R-SQUARE         0.5557
DEP MEAN              710.2958          ADJ R-SQ         0.5481
C.V.                  8.461454

PARAMETER ESTIMATES
                          PARAMETER          STANDARD        T FOR H0:
VARIABLE    DF            ESTIMATE           ERROR           PARAMETER=0    PROB > |T|

INTERCEP     1         41.03583892        36.90989973          1.112          0.2664
TREAT        1         15.60172521         5.94274443          2.625          0.0088
MALE         1         11.34774400        21.89108966          0.518          0.6043
ASIAN        1         32.59769573         8.53577418          3.819          0.0001
BLACK        1         -1.29679273         5.86897429         -0.221          0.8252
HISPANIC     1          3.46794765         6.97586549          0.497          0.6192
FAMSIZE1     1         -1.50089341         0.85297045         -1.760          0.0787
BRTHORDR     1          1.37390221         1.11631250          1.231          0.2187
GRADE1       1          6.60535265         2.75842503          2.395          0.0168
ABSMUCH1     1         -6.18093544         4.09674307         -1.509          0.1316
REPEAT1      1         -9.90114931         4.15704520         -2.382          0.0174
EPOSTHS1     1          3.57402061         4.76848143          0.750          0.4537
LANGPRB1     1          5.72858535         6.26739921          0.914          0.3609
ENJMATH1     1          4.89412084         1.58091449          3.096          0.0020
STDMATH1     1          0.55240625         0.02866084         19.274          0.0001
STDREAD1     1          0.21878939         0.03800742          5.756          0.0001
BOSTON       1         21.16241591         6.44110505          3.286          0.0010
FRESNO       1         28.77507482         6.26229759          4.595          0.0001
PORTLAND     1         25.99492584         5.92377318          4.388          0.0001
SANDIEGO     1         24.52363534         5.83842693          4.200          0.0001
MALE_TRT     1          1.39666373         7.72920845          0.181          0.8566
LAMBDA       1         38.73613809        35.57424307          1.089          0.2764
```

Table D.10

REGRESSION ANALYSIS OF POSTTEST CONTRACEPTIVE KNOWLEDGE FOR COHORT III

DEP VARIABLE: KNOWLDG2
ANALYSIS OF VARIANCE

SOURCE	DF	SUM OF SQUARES	MEAN SQUARE	F VALUE	PROB>F
MODEL	25	5004.50573	200.18023	54.774	0.0001
ERROR	1202	4392.89085	3.65465129		
C TOTAL	1227	9397.39658			

ROOT MSE	1.911714	R-SQUARE	0.5325
DEP MEAN	7.160423	ADJ R-SQ	0.5228
C.V.	26.69834		

PARAMETER ESTIMATES

VARIABLE	DF	PARAMETER ESTIMATE	STANDARD ERROR	T FOR H0: PARAMETER=0	PROB > |T|
INTERCEP	1	-0.09606289	0.95955795	-0.100	0.9203
TREAT	1	2.99491331	0.12379982	24.192	0.0001
MALE	1	-0.96505414	0.54870150	-1.759	0.0789
ASIAN	1	-0.78881643	0.27098445	2.911	0.0037
BLACK	1	-0.14587299	0.18636214	-0.783	0.4339
HISPANIC	1	-0.38502766	0.22597679	-1.704	0.0887
AGE1	1	0.33982722	0.12033576	2.824	0.0048
FEMHD11	1	0.06170019	0.11556811	0.534	0.5935
FEMHDNR	1	1.36538380	1.36698051	0.999	0.3181
BRTHORDR	1	-0.04243297	0.03578052	-1.186	0.2359
LANGPRB1	1	0.03548020	0.19933069	0.178	0.8588
ABSMUCH1	1	0.18101317	0.13368277	1.354	0.1760
SEXED11	1	0.13658638	0.13208717	1.034	0.3013
SEXEDNR	1	0.37459966	0.37679432	0.994	0.3203
EVERSX11	1	0.03438472	0.13412116	0.256	0.7977
EVERSXNR	1	0.35592551	0.34076568	1.044	0.2965
STDREAD1	1	0.005248323	0.001045398	5.020	0.0001
KNOWLDG1	1	0.36996029	0.03006397	12.306	0.0001
AVAILBL1	1	0.13654827	0.03730534	3.660	0.0003
KNOWCON1	1	-0.02091448	0.09572082	-0.218	0.8271
JSPNSBL1	1	0.13217412	0.13564075	0.974	0.3300
BOSTON	1	-0.22812756	0.19239384	-1.186	0.2360
FRESNO	1	0.71558247	0.19602246	3.651	0.0003
PORTLAND	1	0.13589367	0.19568020	0.694	0.4875
SANDIEGO	1	1.29214527	0.18214638	7.094	0.0001
LAMDALS	1	-1.72451068	0.93621571	-1.842	0.0657

Table D.11

REGRESSION ANALYSIS OF POSTTEST KNOWLEDGE OF AVAILABILITY OF CONTRACEPTION FOR COHORT III

```
DEP VARIABLE: AVAILBL2
ANALYSIS OF VARIANCE

SOURCE      DF      SUM OF SQUARES      MEAN SQUARE      F VALUE      PROB>F
MODEL       25        2726.09789        109.04392        45.364       0.0001
ERROR     1202        2889.30520        2.40374809
C TOTAL   1227        5615.40309

ROOT MSE              1.550403          R-SQUARE         0.4855
DEP MEAN              4.619707          ADJ R-SQ         0.4748
C.V.                  33.56063

PARAMETER ESTIMATES

                      PARAMETER         STANDARD         T FOR H0:
VARIABLE    DF        ESTIMATE          ERROR            PARAMETER=0    PROB > |T|

INTERCEP     1      -1.09278060         0.77820266         -1.404         0.1605
TREAT        1       2.09270450         0.10040180         20.843         0.0001
MALE         1      -0.82088558         0.44499758         -1.845         0.0653
ASIAN        1      -0.35371248         0.21976872         -1.609         0.1078
BLACK        1      -0.27980152         0.15113992         -1.851         0.0644
HISPANIC     1      -0.14453341         0.18326745         -0.789         0.4305
AGE1         1       0.23498402         0.09759245          2.408         0.0162
FEMHD11      1      -0.09565580         0.09372588         -1.021         0.3077
FEMHDNR      1      -0.56452875         1.10862285         -0.509         0.6107
BRTHORDR     1      -0.01824794         0.02901804         -0.629         0.5296
LANGPRB1     1      -0.18178293         0.16165743         -1.124         0.2610
ABSMUCH1     1      -0.31168095         0.10841689         -2.875         0.0041
SEXED11      1       0.20110222         0.10712285          1.877         0.0607
SEXEDNR      1       0.31492355         0.30558065          1.031         0.3029
EVERSX11     1       0.08179665         0.10877242          0.752         0.4522
EVERSXNR     1       0.06943072         0.27636138          0.251         0.8017
STDREAD1     1       0.004758437        0.000847819         5.613         0.0001
KNOWLDG1     1       0.11972129         0.02438191          4.910         0.0001
AVAILBL1     1       0.29945155         0.03025467          9.898         0.0001
KNOWCON1     1      -0.004404417        0.07762970         -0.057         0.9548
JSPNSBL1     1       0.02068198         0.11000481          0.188         0.8509
BOSTON       1      -0.22743813         0.15603164         -1.458         0.1452
FRESNO       1       0.25396323         0.15897445          1.598         0.1104
PORTLAND     1       0.01303394         0.15869688          0.082         0.9346
SANDIEGO     1       1.30289352         0.14772093          8.820         0.0001
LAMDALS      1      -0.73204650         0.75927207         -0.964         0.3352
```

Table D.12

REGRESSION ANALYSIS OF POSTTEST KNOWLEDGE OF THE CONSEQUENCES OF ADOLESCENT PREGNANCY FOR COHORT III

```
DEP VARIABLE: KNOWCON2
ANALYSIS OF VARIANCE

SOURCE      DF       SUM OF SQUARES       MEAN SQUARE       F VALUE      PROB>F
MODEL       25        97.27121365         3.89084855        11.078       0.0001
ERROR     1202        422.17097           0.35122377
C TOTAL   1227        519.44218

ROOT MSE              0.5926413           R-SQUARE          0.1873
DEP MEAN              1.165309            ADJ R-SQ          0.1704
C.V.                  50.85699

PARAMETER ESTIMATES

                         PARAMETER            STANDARD        T FOR HO:
VARIABLE     DF          ESTIMATE             ERROR           PARAMETER=0     PROB > |T|

INTERCEP      1          0.25697924           0.29746795          0.864          0.3878
TREAT         1          0.21922027           0.03837859          5.712          0.0001
MALE          1         -0.23349023           0.17010032         -1.373          0.1701
ASIAN         1         -0.003363508          0.08400659         -0.040          0.9681
BLACK         1          0.01800074           0.05777323          0.312          0.7554
HISPANIC      1          0.08251403           0.07005398          1.178          0.2391
AGE1          1         -0.03875769           0.03730471         -1.039          0.2990
FEMHD11       1          0.08879257           0.03582671          2.478          0.0133
FEMHDNR       1          0.05287674           0.42377106          0.125          0.9007
BRTHORDR      1          0.01243779           0.01109215          1.121          0.2624
LANGPRB1      1          0.04800647           0.06179355          0.777          0.4374
ABSMUCH1      1          0.01903302           0.04144235          0.459          0.6461
SEXED11       1         -0.007139548          0.04094771         -0.174          0.8616
SEXEDNR       1         -0.05794736           0.11680820         -0.496          0.6199
EVERSX11      1         -0.11131271           0.04157826         -2.677          0.0075
EVERSXNR      1          0.08001007           0.10563913          0.757          0.4490
STDREAD1      1          0.000669953          0.000324079         2.067          0.0389
KNOWLDG1      1          0.006884024          0.009319986         0.739          0.4603
AVAILBL1      1          0.01050023           0.01156485          0.908          0.3641
KNOWCON1      1          0.25776882           0.02967395          8.687          0.0001
JSPNSBL1      1          0.06687439           0.04204934          1.590          0.1120
BOSTON        1         -0.03527200           0.05964309         -0.591          0.5544
FRESNO        1          0.04653901           0.06076798          0.766          0.4439
PORTLAND      1          0.14260132           0.06066188          2.351          0.0189
SANDIEGO      1          0.41399303           0.05646632          7.332          0.0001
LAMDALS       1         -0.36600569           0.29023174         -1.261          0.2075
```

Table D.13

LOGIT ANALYSIS OF THE PROBABILITY OF BEING SEXUALLY EXPERIENCED AT POSTTEST FOR COHORT III

DEPENDENT VARIABLE: EVERSEX2

1206 OBSERVATIONS
584 EVERSEX2= 0
622 EVERSEX2= 1
22 OBSERVATIONS DELETED DUE TO MISSING VALUES

VARIABLE	BETA	STD. ERROR	CHI-SQUARE	P	R
INTERCEPT	-2.12195785	2.13444915	0.99	0.3202	
TREAT	0.41724173	0.28923394	2.08	0.1491	0.007
AGE1	0.19736525	0.26920660	0.54	0.4635	0.000
MALE	1.47796970	1.36945525	1.16	0.2805	0.000
ASIAN	-1.47866724	0.57491530	6.62	0.0101	-0.053
BLACK	0.43756733	0.41097348	1.13	0.2870	0.000
HISPANIC	-0.17844671	0.49561610	0.13	0.7188	0.000
FAMSIZE1	-0.07403623	0.06566140	1.27	0.2595	0.000
FEMHD11	-0.34129999	0.27233697	1.57	0.2101	0.000
BRTHORDR	0.08703380	0.07780342	1.25	0.2633	0.000
LANGPRB1	0.06377509	0.45869501	0.02	0.8894	0.000
ABSMUCH1	0.65087690	0.28576018	5.19	0.0227	0.044
SEXED11	-0.03423549	0.28166749	0.01	0.9033	0.000
EVERSX11	14.61559418	.	.	.	.
STDREAD1	-0.00055720	0.00229051	0.06	0.8078	0.000
KNOWLDG1	0.05668416	0.06666500	0.72	0.3952	0.000
AVAILBL1	0.08619676	0.08006737	1.16	0.2817	0.000
KNOWCON1	-0.29307043	0.20603695	2.02	0.1549	-0.004
JSPNSBL1	-0.02562684	0.30121313	0.01	0.9322	0.000
BOSTON	-0.42631622	0.46465363	0.84	0.3589	0.000
FRESNO	0.05507126	0.43255209	0.02	0.8987	0.000
PORTLAND	-0.22651999	0.45298958	0.25	0.6170	0.000
SANDIEGO	-0.39125527	0.40600178	0.93	0.3352	0.000
LAMDALS	-0.10431099	2.28727953	0.00	0.9636	0.000

Table D.14

LOGIT ANALYSIS OF THE PROBABILITY OF BEING SEXUALLY ACTIVE DURING THE STEP PROGRAM FOR COHORT III

```
VARIABLE: RCNTSEXB

1135 OBSERVATIONS
 813  RCNTSEXB=  0
 322  RCNTSEXB=  1
  93 OBSERVATIONS DELETED DUE TO MISSING VALUES

VARIABLE          BETA          STD. ERROR       CHI-SQUARE        P        R

INTERCEPT   -1.99604534      1.78648008          1.25       0.2639
TREAT       -0.16251275      0.21340580          0.58       0.4463    0.000
AGE1         0.14573604      0.20730479          0.49       0.4821    0.000
MALE        -1.09282901      1.07772926          1.03       0.3106    0.000
ASIAN       -1.45350194      0.57799228          6.32       0.0119   -0.057
BLACK       -0.55685730      0.30582323          3.32       0.0686   -0.031
HISPANIC    -0.14043753      0.37742812          0.14       0.7098    0.000
FAMSIZE1    -0.06618779      0.05513755          1.44       0.2300    0.000
FEMHD11      0.05718677      0.21457941          0.07       0.7898    0.000
BRTHORDR     0.09492020      0.06579369          2.08       0.1491    0.008
LANGPRB1     0.21018975      0.44251494          0.23       0.6348    0.000
ABSMUCH1     0.35647654      0.21814180          2.67       0.1022    0.022
SEXED11      0.43715265      0.23679385          3.41       0.0649    0.032
EVERSX11     1.82508456      0.25285822         52.10       0.0000    0.192
STDREAD1    -0.00050461      0.00178725          0.08       0.7777    0.000
KNOWLDG1     0.12383058      0.05408410          5.24       0.0220    0.049
AVAILBL1     0.03336759      0.06471157          0.27       0.6061    0.000
KNOWCON1    -0.41298089      0.17148383          5.80       0.0160   -0.053
JSPNSBL1     0.21089264      0.23992096          0.77       0.3794    0.000
BOSTON       0.64756805      0.33297059          3.78       0.0518    0.036
FRESNO       0.09193975      0.34557967          0.07       0.7902    0.000
PORTLAND     0.81852715      0.33232791          6.07       0.0138    0.055
SANDIEGO     0.75009063      0.33116908          5.13       0.0235    0.048
RCNTSEXA     2.46509627      0.24058533        104.99         .        0.276
LAMDALS     -2.77719834      1.80264117          2.37       0.1234   -0.017
```

Table D.15

LOGIT ANALYSIS OF THE PROBABILITY OF USING CONTRACEPTION DURING LAST INTERCOURSE FOR SEXUALLY ACTIVE COHORT III YOUTH

DEPENDENT VARIABLE: CNCONTR2

329 OBSERVATIONS
195 CNCONTR2= 0
134 CNCONTR2= 1
4 OBSERVATIONS DELETED DUE TO MISSING VALUES

VARIABLE	BETA	STD. ERROR	CHI-SQUARE	P	R
INTERCEPT	-3.17066335	2.30163900	1.90	0.1683	
TREAT	0.10955854	0.26891753	0.17	0.6837	0.000
AGE1	0.12939016	0.26914546	0.23	0.6307	0.000
MALE	1.36082691	1.40429514	0.94	0.3325	0.000
BLACK	-0.37554906	0.34742967	1.17	0.2797	0.000
HISPANIC	0.19948553	0.46539942	0.18	0.6682	0.000
FAMSIZE1	-0.08671444	0.07222855	1.44	0.2299	0.000
FEMHD11	-0.09301959	0.28752687	0.10	0.7463	0.000
BRTHORDR	0.08722437	0.08527873	1.05	0.3064	0.000
ABSMUCH1	0.00013259	0.29568431	0.00	13.9996	0.000
SEXED11	0.44482316	0.33324760	1.78	0.1819	0.000
STDREAD1	-0.00070387	0.00235334	0.09	0.7649	0.000
KNOWLDG1	0.07454708	0.06987956	1.14	0.2861	0.000
AVAILBL1	0.02076572	0.08750671	0.06	0.8124	0.000
KNOWCON1	0.04858853	0.23196635	0.04	0.8341	0.000
JSPNSBL1	0.22849003	0.32742461	0.49	0.4853	0.000
BOSTON	0.77673844	0.43431280	3.20	0.0737	0.052
FRESNO	0.19982884	0.47305199	0.18	0.6727	0.000
PORTLAND	0.75306810	0.42964096	3.07	0.0796	0.049
SANDIEGO	-0.07536999	0.45758461	0.03	0.8692	0.000
LAMDALS	2.10496354	2.29715687	0.84	0.3595	0.000
CNCONT11	1.71340643	0.29009900	34.88	0.0000	0.272
CNCONTNR	1.14948259	0.38353306	8.98	0.0027	0.125

Table D.16

LOGIT ANALYSIS OF THE PROBABILITY OF USING CONTRACEPTION DURING LAST INTERCOURSE FOR SEXUALLY ACTIVE COHORT III MALES

DEPENDENT VARIABLE: CNCONTR2

329 OBSERVATIONS
195 CNCONTR2= 0
134 CNCONTR2= 1
4 OBSERVATIONS DELETED DUE TO MISSING VALUES

VARIABLE	BETA	STD. ERROR	CHI-SQUARE	P	R
INTERCEPT	-4.08210907	2.50791430	2.65	0.1036	
TREAT	0.53416428	0.53487156	1.00	0.3180	0.000
AGE1	0.07995119	0.27438529	0.08	0.7708	0.000
MALE	2.40470454	1.80666257	1.77	0.1832	0.000
BLACK	-0.33218816	0.35062027	0.90	0.3434	0.000
HISPANIC	0.16545981	0.46828133	0.12	0.7238	0.000
FAMSIZE1	-0.08744333	0.07245147	1.46	0.2275	0.000
FEMHD11	-0.07440807	0.28795734	0.07	0.7961	0.000
BRTHORDR	0.09128563	0.08564176	1.14	0.2865	0.000
ABSMUCH1	0.00662312	0.29580719	0.00	0.9821	0.000
SEXED11	0.44605317	0.33225476	1.80	0.1794	0.000
STDREAD1	-0.00080941	0.00235477	0.12	0.7310	0.000
KNOWLDG1	0.07604310	0.07010336	1.18	0.2780	0.000
AVAILBL1	0.02022703	0.08762853	0.05	0.8174	0.000
KNOWCON1	0.06463766	0.23309743	0.08	0.7816	0.000
JSPNSBL1	0.22062125	0.32789958	0.45	0.5011	0.000
BOSTON	0.74439501	0.43607644	2.91	0.0878	0.045
FRESNO	0.20608358	0.47445181	0.19	0.6640	0.000
PORTLAND	0.68041157	0.43598470	2.44	0.1186	0.031
SANDIEGO	-0.07300275	0.45793364	0.03	0.8733	0.000
LAMDALS	3.35234659	2.67131991	1.57	0.2095	0.000
CNCONT11	1.73238053	0.29188341	35.23	0.0000	0.273
CNCONTNR	1.15471973	0.38435866	9.03	0.0027	0.126
MALE_T	-0.58676621	0.63465869	0.85	0.3552	0.000

Table D.17

LOGIT ANALYSIS OF THE PROBABILITY OF USING CONTRACEPTION DURING LAST INTERCOURSE FOR SEXUALLY ACTIVE COHORT III FEMALES

DEPENDENT VARIABLE: CNCONTR2

329 OBSERVATIONS
195 CNCONTR2= 0
134 CNCONTR2= 1
4 OBSERVATIONS DELETED DUE TO MISSING VALUES

VARIABLE	BETA	STD. ERROR	CHI-SQUARE	P	R
INTERCEPT	-1.67740453	1.79597820	0.87	0.3503	
TREAT	-0.05260193	0.32082215	0.03	0.8698	0.000
AGE1	0.07995119	0.27438529	0.08	0.7708	0.000
FEMALE	-2.40470454	1.80666257	1.77	0.1832	0.000
BLACK	-0.33218816	0.35062027	0.90	0.3434	0.000
HISPANIC	0.16545981	0.46828133	0.12	0.7238	0.000
FAMSIZE1	-0.08744333	0.07245147	1.46	0.2275	0.000
FEMHD11	-0.07440807	0.28795734	0.07	0.7961	0.000
BRTHORDR	0.09128563	0.08564176	1.14	0.2865	0.000
FEMALE_T	0.58676621	0.63465869	0.85	0.3552	0.000
ABSMUCH1	0.00662312	0.29580719	0.00	0.9821	0.000
SEXED11	0.44605317	0.33225476	1.80	0.1794	0.000
STDREAD1	-0.00080941	0.00235477	0.12	0.7310	0.000
KNOWLDG1	0.07604310	0.07010336	1.18	0.2780	0.000
AVAILBL1	0.02022703	0.08762853	0.05	0.8174	0.000
KNOWCON1	0.06463766	0.23309743	0.08	0.7816	0.000
JSPNSBL1	0.22062125	0.32789958	0.45	0.5011	0.000
BOSTON	0.74439501	0.43607644	2.91	0.0878	0.045
FRESNO	0.20608358	0.47445181	0.19	0.6640	0.000
PORTLAND	0.68041157	0.43598470	2.44	0.1186	0.031
SANDIEGO	-0.07300275	0.45793364	0.03	0.8733	0.000
LAMDALS	3.35234659	2.67131991	1.57	0.2095	0.000
CNCONT11	1.73238053	0.29188341	35.23	0.0000	0.273
CNCONTNR	1.15471973	0.38435866	9.03	0.0027	0.126

Table D.18

REGRESSION ANALYSIS OF SPRING 1987 READING SCORES (IN NCES) WITH SITE-TREATMENT INTERACTION TERMS FOR COHORT II YOUTH (PORTLAND EXCLUDED)

```
DEP VARIABLE: RNCE     Normal Curve Equivalent of RSCORE
ANALYSIS OF VARIANCE

SOURCE     DF       SUM OF SQUARES      MEAN SQUARE     F VALUE      PROB>F
MODEL      20       97668.93044         4883.44652      43.247       0.0001
ERROR     743       83899.94778         112.92052
C TOTAL   763       181568.88

ROOT MSE            10.62641            R-SQUARE        0.5379
DEP MEAN            43.05393            ADJ R-SQ        0.5255
C.V.                24.68162


PARAMETER ESTIMATES

                        PARAMETER          STANDARD         T FOR H0:
VARIABLE     DF         ESTIMATE           ERROR         PARAMETER=0     PROB > |T|

INTERCEP      1       -83.86397872       6.80896156        -12.317         0.0001
TREAT         1        -0.80703815       1.62045543         -0.498         0.6186
BOS_T         1        -1.37531932       2.19696746         -0.626         0.5315
FRESNO_T      1        -2.44027776       2.23077875         -1.094         0.2743
DIEGO_T       1        -0.72449946       2.27141553         -0.319         0.7498
BLACK         1         0.05290759       1.31459416          0.040         0.9679
HISPANIC      1        -0.06306438       1.47519996         -0.043         0.9659
ASIAN         1        -0.22567839       1.64359763         -0.137         0.8908
BOSTON        1        -5.79505899       1.59985864         -3.622         0.0003
FRESNO        1        -8.99462239       1.63591530         -5.498         0.0001
SANDIEGO      1        -2.35317926       1.65498150         -1.422         0.1555
STDREAD1      1         0.16704442       0.007068096        23.634         0.0001
ENJREAD1      1        -0.51968926       0.3800639          -1.367         0.1719
REPEAT1       1        -1.30619246       0.93373252         -1.399         0.1623
GRADE1        1         1.33136092       0.57340649          2.322         0.0205
ABSMUCH1      1         0.93999532       1.04222052          0.902         0.3674
EPOSTHS1      1         1.65544997       0.91174025          1.816         0.0698
FAMSIZE1      1         0.06513191       0.20260895          0.321         0.7479
BRTHORDR      1         0.15508794       0.24624680          0.630         0.5290
LANGPRB1      1        -1.08239447       1.19654148         -0.905         0.3660
MALE          1         0.71993217       0.79886614          0.901         0.3678
```

Table D.19

REGRESSION ANALYSIS OF SPRING 1987 READING SCORES WITH RACIAL/ETHNIC- AND GENDER-TREATMENT INTERACTION TERMS FOR COHORT II YOUTH (PORTLAND EXCLUDED)

DEP VARIABLE: RNCE   Normal Curve Equivalent of RSCORE
ANALYSIS OF VARIANCE

SOURCE	DF	SUM OF SQUARES	MEAN SQUARE	F VALUE	PROB>F
MODEL	21	97768.29683	4655.63318	41.223	0.0001
ERROR	742	83800.58139	112.93879		
C TOTAL	763	181568.88			

ROOT MSE	10.62727	R-SQUARE	0.5385
DEP MEAN	43.05393	ADJ R-SQ	0.5254
C.V.	24.68362		

PARAMETER ESTIMATES

VARIABLE	DF	PARAMETER ESTIMATE	STANDARD ERROR	T FOR H0: PARAMETER=0	PROB > |T|
INTERCEP	1	-83.44946113	6.85416202	-12.175	0.0001
TREAT	1	-2.36857221	2.52278777	-0.939	0.3481
BLACK_T	1	1.65453783	2.63639399	0.628	0.5305
HISP_T	1	-0.30900149	2.88956425	-0.107	0.9149
ASIAN_T	1	1.58105564	2.88043727	0.549	0.5832
MALE_T	1	-1.36302765	1.56677083	-0.870	0.3846
BLACK	1	-0.88243081	1.79102591	-0.493	0.6224
HISPANIC	1	-0.04606046	2.00701039	-0.023	0.9817
ASIAN	1	-1.08979201	2.15222586	-0.506	0.6128
BOSTON	1	-6.49641976	1.17477576	-5.530	0.0001
FRESNO	1	-10.21628823	1.20194553	-8.500	0.0001
SANDIEGO	1	-2.76970586	1.16071426	-2.386	0.0173
STDREAD1	1	0.16731703	0.007070987	23.662	0.0001
ENJREAD1	1	-0.53067884	0.38040990	-1.395	0.1634
REPEAT1	1	-1.22961064	0.93430212	-1.316	0.1886
GRADE1	1	1.34732221	0.57470766	2.344	0.0193
ABSMUCH1	1	0.90681334	1.04185901	0.870	0.3844
EPOSTHS1	1	1.72157154	0.91463297	1.882	0.0602
FAMSIZE1	1	0.07994077	0.20379464	0.392	0.6950
BRTHORDR	1	0.17260081	0.24620639	0.701	0.4835
LANGPRB1	1	-1.08887256	1.19240312	-0.913	0.3614
MALE	1	1.37600623	1.12549869	1.223	0.2219

Table D.20

REGRESSION ANALYSIS OF SPRING 1987 MATH SCORES WITH SITE-TREATMENT INTERACTION TERMS FOR COHORT II YOUTH (PORTLAND EXCLUDED)

```
DEP VARIABLE: MNCE      Normal Curve Equivalent of MSCORE
ANALYSIS OF VARIANCE

SOURCE      DF       SUM OF SQUARES        MEAN SQUARE       F VALUE      PROB>F
MODEL       21         231003.06           11000.14588       58.895       0.0001
ERROR      758         141575.08           186.77451
C TOTAL    779         372578.14

ROOT MSE                13.66655           R-SQUARE          0.6200
DEP MEAN                53.99513           ADJ R-SQ          0.6095
C.V.                    25.3107


PARAMETER ESTIMATES

                          PARAMETER          STANDARD        T FOR HO:
VARIABLE    DF            ESTIMATE             ERROR        PARAMETER=0     PROB > |T|

INTERCEP     1          -76.75395889       8.87440221          -8.649         0.0001
TREAT        1           -0.90868178       2.07698189          -0.438         0.6619
BOS_T        1            2.32099049       2.80481913           0.828         0.4082
FRESNO_T     1            1.06655660       2.86048979           0.373         0.7094
DIEGO_T      1            1.47433388       2.87744133           0.512         0.6085
STDMATH1     1            0.14207502       0.009012863         15.764         0.0001
ENJMATH1     1            1.85512674       0.45131171           4.111         0.0001
STDREAD1     1            0.03775237       0.01042749           3.620         0.0003
REPEAT1      1           -3.51187937       1.19770038          -2.932         0.0035
GRADE1       1           -0.10502854       0.73079798          -0.144         0.8858
ABSMUCH1     1           -1.67306683       1.31057909          -1.277         0.2021
EPOSTHS1     1           -0.18679086       1.16825243          -0.160         0.8730
FAMSIZE1     1           -0.63913295       0.26109020          -2.448         0.0146
BRTHORDR     1            0.37553323       0.31375737           1.197         0.2317
LANGPRB1     1            2.25279270       1.52159481           1.481         0.1391
MALE         1            0.48766003       1.00476626           0.485         0.6276
BLACK        1           -0.03648525       1.66930044          -0.022         0.9826
HISPANIC     1            1.52613547       1.87706931           0.813         0.4164
ASIAN        1           12.09215536       2.11018829           5.730         0.0001
BOSTON       1           -9.08644992       2.05118077          -4.430         0.0001
FRESNO       1           -4.03050034       2.10774519          -1.912         0.0562
SANDIEGO     1            1.72010838       2.10229521           0.818         0.4135
```

Table D.21

REGRESSION ANALYSIS OF SPRING 1987 MATH SCORES WITH RACIAL/ETHNIC- AND GENDER-TREATMENT INTERACTION TERMS FOR COHORT II YOUTH (PORTLAND EXCLUDED)

DEP VARIABLE: MNCE Normal Curve Equivalent of MSCORE
ANALYSIS OF VARIANCE

SOURCE	DF	SUM OF SQUARES	MEAN SQUARE	F VALUE	PROB>F
MODEL	22	231062.21	10502.82790	56.182	0.0001
ERROR	757	141515.93	186.94310		
C TOTAL	779	372578.14			

ROOT MSE	13.67271	R-SQUARE	0.6202
DEP MEAN	53.99513	ADJ R-SQ	0.6091
C.V.	25.32212		

PARAMETER ESTIMATES

VARIABLE	DF	PARAMETER ESTIMATE	STANDARD ERROR	T FOR H0: PARAMETER=0	PROB > |T|
INTERCEP	1	-77.11587567	8.91499792	-8.650	0.0001
TREAT	1	0.18624241	3.20498407	0.058	0.9537
BLACK_T	1	1.00166596	3.33645197	0.300	0.7641
HISP_T	1	-1.57364434	3.67618673	-0.428	0.6687
ASIAN_T	1	-0.28091468	3.65503345	-0.077	0.9388
MALE_T	1	0.25395147	2.00001101	0.127	0.8990
STDMATH1	1	0.14206993	0.009030693	15.732	0.0001
MATH1	1	1.85662452	0.45203937	4.107	0.0001
STDREAD1	1	0.03775431	0.01045184	3.612	0.0003
REPEAT1	1	-3.45341841	1.19870785	-2.881	0.0041
GRADE1	1	-0.12725366	0.73219169	-0.174	0.8621
ABSMUCH1	1	-1.80535696	1.31270255	-1.375	0.1694
EPOSTHS	1	-0.22213828	1.17230228	-0.189	0.8498
FAMSIZE1	1	-0.63049756	0.26246551	-2.402	0.0165
BRTHORDR	1	0.37040138	0.31393404	1.180	0.2384
LANGPRB1	1	2.36069926	1.51734414	1.556	0.1202
MALE	1	0.28303472	1.43134101	0.198	0.8433
BLACK	1	-0.47596533	2.29532854	-0.207	0.8358
HISPANIC	1	2.43788233	2.56362526	0.951	0.3419
ASIAN	1	12.23512834	2.77188302	4.414	0.0001
BOSTON	1	-7.94836290	1.50240150	-5.290	0.0001
FRESNO	1	-3.54164915	1.54400314	-2.294	0.0221
SANDIEGO	1	2.37526107	1.48937647	1.595	0.1112

Table D.22

REGRESSION ANALYSIS OF THE PERCENTAGE OF SY 1986-87 ABSENT WITH SITE-TREATMENT INTERACTION TERMS FOR COHORT II YOUTH (SAN DIEGO EXCLUDED)

```
DEP VARIABLE: PCTABS
ANALYSIS OF VARIANCE

SOURCE      DF     SUM OF SQUARES     MEAN SQUARE     F VALUE     PROB>F
MODEL       22       2.15646655       0.09802121      7.347       0.0001
ERROR      936      12.48768786       0.01334155
C TOTAL    958      14.64415441

ROOT MSE             0.1155056        R-SQUARE        0.1473
DEP MEAN             0.1182426        ADJ  R-SQ       0.1272
C.V.                 97.68525

PARAMETER ESTIMATES

                      PARAMETER          STANDARD        T FOR H0:
VARIABLE     DF       ESTIMATE           ERROR           PARAMETER=0    PROB > |T|

INTERCEP      1       0.17433003        0.06767328          2.576        0.0101
TREAT         1       0.01279614        0.01524293          0.839        0.4014
BOS_T         1      -0.02681912        0.02114343         -1.268        0.2050
FRESNO_T      1      -0.01077589        0.02160215         -0.499        0.6180
PORT_T        1       0.005850614       0.02143447          0.273        0.7849
STDREAD1      1      -0.000113124       0.000080369        -1.408        0.1596
STDMATH1      1      -0.000043338       0.000070849        -0.612        0.5409
ENJREAD1      1      -0.001709861       0.003692976        -0.463        0.6435
ENJMATH1      1      -0.002576081       0.003457951        -0.745        0.4565
REPEAT1       1       0.01097566        0.008926118         1.230        0.2192
GRADE1        1       0.005971574       0.005765731         1.036        0.3006
ABSMUCH1      1       0.06777371        0.009164661         7.395        0.0001
EPOSTHS1      1      -0.002436860       0.008827171        -0.276        0.7826
FAMSIZE1      1       0.000497493       0.002020700         0.246        0.8056
BRTHORDR      1       0.002525199       0.002369388         1.066        0.2868
LANGPRB1      1       0.001924896       0.01320956          0.146        0.8842
MALE          1      -0.01423648        0.007757564        -1.835        0.0668
BLACK         1      -0.01253681        0.01069477         -1.172        0.2414
HISPANIC      1       0.01140868        0.01380528          0.826        0.4088
ASIAN         1      -0.07105985        0.01583622         -4.487        0.0001
BOSTON        1       0.03611242        0.01541022          2.343        0.0193
FRESNO        1      -0.005307401       0.01607076         -0.330        0.7413
PORTLAND      1       0.01452246        0.01523120          0.953        0.3406
```

Table D.23

REGRESSION ANALYSIS OF THE PERCENTAGE OF SY 1986-87 ABSENT WITH RACIAL/ETHNIC- AND GENDER-TREATMENT INTERACTION TERMS FOR COHORT II YOUTH (SAN DIEGO EXCLUDED)

```
DEP VARIABLE: PCTABS
ANALYSIS OF VARIANCE

SOURCE      DF      SUM OF SQUARES      MEAN SQUARE       F VALUE      PROB>F
MODEL       23        2.17871337        0.09472667        7.105        0.0001
ERROR      935       12.46544104        0.01333202
C TOTAL    958       14.64415441

ROOT MSE              0.1154644         R-SQUARE          0.1488
MEAN                  0.1182426         ADJ R-SQ          0.1278
C.V.                  97.65038

PARAMETER ESTIMATES

                       PARAMETER          STANDARD         T FOR H0:
VARIABLE    DF         ESTIMATE            ERROR          PARAMETER=0     PROB > |T|

INTERCEP     1        0.15993812        0.06789706          2.356           0.0187
TREAT        1        0.03626593        0.01974011          1.837           0.0665
BLACK_T      1       -0.03285159        0.02077577         -1.581           0.1142
HISP_T       1       -0.05170522        0.02521014         -2.051           0.0405
ASIAN_T      1       -0.02632532        0.02633153         -1.000           0.3177
MALE_T       1       -0.005456258       0.01522184         -0.358           0.7201
STDREAD1     1       -0.000107397       0.000080409        -1.336           0.1820
STDMATH1     1       -0.000042038       0.000070879        -0.593           0.5533
ENJREAD1     1       -0.001797176       0.003688059        -0.487           0.6262
ENJMATH1     1       -0.002523630       0.003462959        -0.729           0.4663
REPEAT1      1        0.01104223        0.008915900         1.238           0.2158
GRADE1       1        0.005938425       0.005768599         1.029           0.3035
ABSMUCH1     1        0.06797292        0.009167798         7.414           0.0001
EPOSTHS1     1       -0.003299705       0.008856000        -0.373           0.7095
FAMSIZE1     1        0.000640975       0.002023083         0.317           0.7514
BRTHORDR     1        0.002376950       0.002371197         1.002           0.3164
LANGPRB1     1        0.001909278       0.01325387          0.144           0.8855
MALE         1       -0.01286225        0.01072380         -1.199           0.2307
BLACK        1        0.002127548       0.01450477          0.147           0.8834
HISPANIC     1        0.03526006        0.01867236          1.888           0.0593
ASIAN        1       -0.05952669        0.02068270         -2.878           0.0041
BOSTON       1        0.02382574        0.01141534          2.087           0.0371
FRESNO       1       -0.009689882       0.01191878         -0.813           0.4164
PORTLAND     1        0.01750751        0.01098567          1.594           0.1113
```

Table D.24

REGRESSION ANALYSIS OF THE PERCENTAGE OF HIGH SCHOOL CREDITS EARNED IN SY 1986-87 WITH SITE-TREATMENT INTERACTION TERMS FOR COHORT II YOUTH (ALL SITES INCLUDED)

DEP VARIABLE: PCTCRED
ANALYSIS OF VARIANCE

SOURCE	DF	SUM OF SQUARES	MEAN SQUARE	F VALUE	PROB>F
MODEL	35	10.99022540	0.31400644	34.249	0.0001
ERROR	1008	9.24176825	0.009168421		
C TOTAL	1043	20.23199364			
ROOT MSE		0.09575187	R-SQUARE	0.5432	
DEP MEAN		0.2203054	ADJ R-SQ	0.5273	
C.V.		43.46325			

PARAMETER ESTIMATES

VARIABLE	DF	PARAMETER ESTIMATE	STANDARD ERROR	T FOR H0: PARAMETER=0	PROB > |T|
INTERCEP	1	-0.25016548	0.07064528	-3.541	0.0004
TREAT	1	0.009583160	0.01369886	0.700	0.4844
BOS_T	1	0.008040101	0.01959471	0.410	0.6817
FRESNO_T	1	-0.001131478	0.01922070	-0.059	0.9531
PORT_T	1	-0.03017203	0.01880364	-1.605	0.1089
DIEGO_T	1	-0.008918624	0.01902941	-0.469	0.6394
BOSTON	1	-0.86132462	0.11605620	-7.422	0.0001
FRESNO	1	0.09019568	0.01440569	6.261	0.0001
PORTLAND	1	0.11788173	0.01338019	8.810	0.0001
SANDIEGO	1	-0.82100232	0.11789875	-6.964	0.0001
STRD1_CM	1	0.000289569	0.000090029	3.216	0.0013
EJRD1_CM	1	-0.01023789	0.004770306	-2.146	0.0321
RPT1_CM	1	-0.04073536	0.01129812	-3.605	0.0003
GRD1_CM	1	0.15023373	0.008014887	18.744	0.0001
ABS1_CM	1	-0.07371549	0.01337543	-5.511	0.0001
EPHS1_CM	1	-0.002207511	0.01121576	-0.197	0.8440
FMSZ1_CM	1	-0.000537563	0.002362721	-0.228	0.8201
TODR_CM	1	-0.001666781	0.003019271	-0.552	0.5810
LGPB1_CM	1	-0.006333901	0.01474655	-0.430	0.6676
MALE_CM	1	-0.02542401	0.01000530	-2.541	0.0112
STRD1_AN	1	0.000208617	0.000067192	3.105	0.0020
EJRD1_AN	1	0.002261052	0.003883780	0.582	0.5606
RPT1_AN	1	-0.000147594	0.009453231	-0.016	0.9875
GRD1_AN	1	0.02224787	0.006373214	3.491	0.0005
ABS1_AN	1	-0.02786974	0.009210132	-3.026	0.0025
EPHS1_AN	1	0.005713281	0.009342710	0.612	0.5410
FMSZ1_AN	1	-0.000695668	0.002006225	-0.347	0.7288
BTODR_AN	1	0.001081140	0.002429481	0.445	0.6564
LGPB1_AN	1	0.02115671	0.01324494	1.597	0.1105
MALE_AN	1	-0.003742097	0.007950012	-0.471	0.6380
BLCK_CM	1	-0.01736005	0.01638642	-1.059	0.2897
HISP_CM	1	-0.02710907	0.01881373	-1.441	0.1499
ASIAN_CM	1	0.04685107	0.02171854	2.157	0.0312
BLCK_AN	1	0.007000445	0.01075374	0.651	0.5152
HISP_AN	1	-0.01490261	0.01463095	-1.019	0.3087
ASIAN_AN	1	0.03273149	0.01510660	2.167	0.0305

Table D.25

REGRESSION ANALYSIS OF THE PERCENTAGE OF HIGH SCHOOL CREDITS EARNED IN SY 1986-87 WITH RACIAL/ETHNIC- AND GENDER-TREATMENT INTERACTION TERMS FOR COHORT II YOUTH
(ALL SITES INCLUDED)

```
DEP VARIABLE: PCTCRED
ANALYSIS OF VARIANCE

SOURCE      DF       SUM OF SQUARES      MEAN SQUARE      F VALUE      PROB>F
MODEL       31         8.91692092        0.28764261       25.726       0.0001
ERROR     1012        11.31507272        0.01118090
C TOTAL   1043        20.23199364

ROOT MSE               0.1057398         R-SQUARE          0.4407
DEP MEAN               0.2203054         ADJ R-SQ          0.4236
C.V.                  47.99692

PARAMETER ESTIMATES

                          PARAMETER          STANDARD        T FOR HO:
VARIABLE     DF           ESTIMATE            ERROR        PARAMETER=0     PROB > |T|

INTERCEP      1         -0.49848991        0.06031941         -8.264          0.0001
TREAT         1         -0.02142469        0.01820578         -1.177          0.2395
BLACK_T       1          0.03751380        0.01931412          1.942          0.0524
HISP_T        1          0.02275016        0.02292618          0.992          0.3213
ASIAN_T       1          0.02467510        0.02256985          1.093          0.2745
MALE_T        1         -0.008255905       0.01337361         -0.617          0.5372
STRD1_CM      1         -0.000096175       0.000086097        -1.117          0.2642
EJRD1_CM      1         -0.01338779        0.005274200        -2.538          0.0113
RPT1_CM       1         -0.06078908        0.01227489         -4.952          0.0001
GRD1_CM       1          0.11933772        0.007303697        16.339          0.0001
ABS1_CM       1         -0.06823193        0.01472417         -4.634          0.0001
EPHS1_CM      1          0.001104754       0.01236597          0.089          0.9288
FMSZ1_CM      1         -0.002304194       0.002577165        -0.894          0.3715
BTODR_CM      1         -0.005627225       0.003305118        -1.703          0.0890
LGPB1_CM      1         -0.01496431        0.01626802         -0.920          0.3579
MALE_CM       1         -0.03131982        0.01301653         -2.406          0.0163
STRD1_AN      1          0.000432605       0.000069699         6.207          0.0001
EJRD1_AN      1          0.004504421       0.004270392         1.055          0.2918
RPT1_AN       1          0.006637162       0.01023719          0.648          0.5169
GRD1_AN       1          0.03928694        0.006304637         6.231          0.0001
ABS1_AN       1         -0.03190745        0.01015809         -3.141          0.0017
EPHS1_AN      1          0.01174109        0.01030756          1.139          0.2549
FMSZ1_AN      1          0.001802558       0.002193988         0.822          0.4115
BTODR_AN      1          0.001580834       0.002677700         0.590          0.5551
LGPB1_AN      1          0.02566066        0.01471892          1.743          0.0816
MALE_AN       1          0.004605663       0.01088338          0.423          0.6723
BLCK_CM       1         -0.04637705        0.01990833         -2.330          0.0200
HISP_CM       1         -0.04392295        0.02331890         -1.884          0.0599
ASIAN_CM      1          0.05316883        0.02578467          2.062          0.0395
BLCK_AN       1         -0.02419566        0.01497176         -1.616          0.1064
HISP_AN       1         -0.01461848        0.01835176         -0.797          0.4259
ASIAN_AN      1         -0.006494858       0.02004147         -0.324          0.7460
```

Table D.26

LOGIT ANALYSIS OF THE PROBABILITY OF PROMOTION AFTER SY 1986-87 FOR COHORT II YOUTH (BOSTON AND SEATTLE)

DEPENDENT VARIABLE: PROMOTE

497 OBSERVATIONS
 59 PROMOTE = 0
438 PROMOTE = 1
164 OBSERVATIONS DELETED DUE TO MISSING VALUES

VARIABLE	BETA	STD. ERROR	CHI-SQUARE	P	R
INTERCEPT	0.15830859	3.15256677	0.00	0.9600	
TREAT	-0.07659272	0.31466147	0.06	0.8077	0.000
STDREAD1	0.00761578	0.00328693	5.37	0.0205	0.096
STDMATH1	-0.00257133	0.00290227	0.78	0.3756	0.000
ENJREAD1	-0.08996299	0.15025766	0.36	0.5494	0.000
ENJMATH1	0.12076016	0.13441086	0.81	0.3690	0.000
REPEAT1	0.23103634	0.34317231	0.45	0.5008	0.000
GRADE1	-0.00675149	0.26923505	0.00	0.9800	0.000
ABSMUCH1	-1.35355155	0.34041427	15.81	0.0001	-0.195
EPOSTHS1	-0.60536151	0.36127713	2.81	0.0938	-0.047
FAMSIZE1	0.07995810	0.09044151	0.78	0.3766	0.000
BRTHORDR	-0.08464422	0.09594358	0.78	0.3777	0.000
LANGPRB1	-0.37785519	0.60979606	0.38	0.5355	0.000
MALE	-0.41956793	0.32961282	1.62	0.2030	0.000
BLACK	0.89397496	0.40540585	4.86	0.0274	0.089
HISPANIC	0.77754661	0.53125928	2.14	0.1433	0.020
ASIAN	2.68404562	1.17705915	5.20	0.0226	0.094
BOSTON	-2.39728860	0.53254212	20.26	0.0000	-0.225

Table D.27

LOGIT ANALYSIS OF THE PROBABILITY OF PROMOTION AFTER SY 1986-87 WITH SITE-TREATMENT INTERACTION TERMS FOR COHORT II YOUTH (BOSTON AND SEATTLE)

DEPENDENT VARIABLE: PROMOTE

497 OBSERVATIONS
59 PROMOTE = 0
438 PROMOTE = 1
164 OBSERVATIONS DELETED DUE TO MISSING VALUES

VARIABLE	BETA	STD. ERROR	CHI-SQUARE	P	R
INTERCEPT	0.22785046	3.17512877	0.01	0.9428	
STDREAD1	0.00762116	0.00328419	5.39	0.0203	0.097
STDMATH1	-0.00255723	0.00290058	0.78	0.3780	0.000
ENJREAD1	-0.08964747	0.15029981	0.36	0.5509	0.000
ENJMATH1	0.12074044	0.13436512	0.81	0.3689	0.000
REPEAT1	0.23007928	0.34320045	0.45	0.5026	0.000
GRADE1	-0.00544129	0.26919102	0.00	0.9839	0.000
ABSMUCH1	-1.35103327	0.34043250	15.75	0.0001	-0.195
EPOSTHS1	-0.60375880	0.36137914	2.79	0.0948	-0.047
FAMSIZE1	0.07977040	0.09044876	0.78	0.3778	0.000
BRTHORDR	-0.08405800	0.09595576	0.77	0.3810	0.000
LANGPRB1	-0.38042078	0.60941286	0.39	0.5325	0.000
MALE	-0.41766444	0.32975031	1.60	0.2053	0.000
BLACK	0.89210992	0.40526194	4.85	0.0277	0.089
HISPANIC	0.77627772	0.53094897	2.14	0.1437	0.019
ASIAN	2.68097662	1.17645871	5.19	0.0227	0.094
BOSTON	-2.50403607	0.78110943	10.28	0.0013	-0.151
BOS_T	-0.05456772	0.33464643	0.03	0.8705	0.000
SEAT_T	-0.24635042	0.93650284	0.07	0.7925	0.000

Table D.28

LOGIT ANALYSIS OF THE PROBABILITY OF PROMOTION AFTER SY 1986-87 WITH RACIAL/ETHNIC-TREATMENT INTERACTION TERMS FOR COHORT II YOUTH
(BOSTON AND SEATTLE)

DEPENDENT VARIABLE: PROMOTE

497 OBSERVATIONS
59 PROMOTE = 0
438 PROMOTE = 1
164 OBSERVATIONS DELETED DUE TO MISSING VALUES

VARIABLE	BETA	STD. ERROR	CHI-SQUARE	P	R
INTERCEPT	0.24938472	3.17198242	0.01	0.9373	
STDREAD1	0.00785448	0.00330895	5.63	0.0176	0.100
STDMATH1	-0.00266385	0.00292673	0.83	0.3627	0.000
ENJREAD1	-0.09976921	0.15233420	0.43	0.5125	0.000
ENJMATH1	0.12147403	0.13743459	0.78	0.3768	0.000
REPEAT1	0.24268887	0.34398911	0.50	0.4805	0.000
GRADE1	-0.02045527	0.27352054	0.01	0.9404	0.000
ABSMUCH1	-1.36389830	0.34374237	15.74	0.0001	-0.195
EPOSTHS1	-0.57206145	0.36798006	2.42	0.1200	-0.034
FAMSIZE1	0.07869411	0.09066235	0.75	0.3854	0.000
BRTHORDR	-0.07804723	0.09601620	0.66	0.4163	0.000
LANGPRB1	-0.36200921	0.62846096	0.33	0.5646	0.000
MALE	-0.40764668	0.33343744	1.49	0.2215	0.000
BLACK	0.75615611	0.55872301	1.83	0.1759	0.000
HISPANIC	0.56274340	0.70585846	0.64	0.4253	0.000
ASIAN	8.10275794	22.18098657	0.13	0.7149	0.000
BOSTON	-2.40893439	0.53414170	20.34	0.0000	-0.225
BLACK_T	0.01704425	0.43233562	0.00	0.9686	0.000
HISP_T	0.14081094	0.70199281	0.04	0.8410	0.000
ASIAN_T	-6.27982297	22.19859083	0.08	0.7773	0.000
OTHER_T	-0.28075321	0.67013999	0.18	0.6753	0.000

Table D.29

LOGIT ANALYSIS OF THE PROBABILITY OF PROMOTION AFTER SY 1986-87 WITH GENDER-TREATMENT INTERACTION TERMS FOR COHORT II YOUTH (BOSTON AND SEATTLE)

```
DEPENDENT VARIABLE: PROMOTE

497 OBSERVATIONS
 59  PROMOTE =  0
438  PROMOTE =  1
164 OBSERVATIONS DELETED DUE TO MISSING VALUES

VARIABLE           BETA        STD. ERROR      CHI-SQUARE        P        R

INTERCEPT   -0.29439874     3.18945009            0.01      0.9265
STDREAD1     0.00784755     0.00331691            5.60      0.0180    0.100
STDMATH1    -0.00266979     0.00291738            0.84      0.3601    0.000
ENJREAD1    -0.07300483     0.15216270            0.23      0.6314    0.000
ENJMATH1     0.13361998     0.13518504            0.98      0.3229    0.000
REPEAT1      0.26773631     0.34690325            0.60      0.4402    0.000
GRADE1      -0.00194763     0.26994965            0.00      0.9942    0.000
ABSMUCH1    -1.37676342     0.34163168           16.24      0.0001   -0.198
EPOSTHS1    -0.53842436     0.36546826            2.17      0.1407   -0.022
FAMSIZE1     0.07855488     0.09145986            0.74      0.3904    0.000
BRTHORDR    -0.07623054     0.09597308            0.63      0.4270    0.000
LANGPRB1    -0.34138111     0.61224933            0.31      0.5771    0.000
MALE        -0.02114933     0.45989512            0.00      0.9633    0.000
BLACK        0.84313493     0.40669495            4.30      0.0382    0.080
HISPANIC     0.71117126     0.53383794            1.77      0.1828    0.000
ASIAN        2.60389700     1.16937208            4.96      0.0260    0.090
BOSTON      -2.41050000     0.53460435           20.33      0.0000   -0.225
MALE_T      -0.46879627     0.44807858            1.09      0.2955    0.000
FEMALE_T     0.32878535     0.45513405            0.52      0.4701    0.000
```

Table D.30

LOGIT ANALYSIS OF THE RESTRICTIVE MEASURE OF DROPOUT AFTER SY 1986-87 FOR COHORT II YOUTH (ALL SITES INCLUDED)

DEPENDENT VARIABLE: DROPOUT

1393 OBSERVATIONS
1318 DROPOUT = 0
75 DROPOUT = 1
240 OBSERVATIONS DELETED DUE TO MISSING VALUES

VARIABLE	BETA	STD. ERROR	CHI-SQUARE	P	R
INTERCEPT	-4.33351346	2.45473232	3.12	0.0775	
TREAT	-0.22496953	0.25684957	0.77	0.3811	0.000
STDREAD1	-0.00536624	0.00301073	3.18	0.0747	-0.045
STDMATH1	0.00388723	0.00234281	2.75	0.0971	0.036
MALE	-0.25232378	0.26595690	0.90	0.3428	0.000
ENJREAD1	-0.05910162	0.13733498	0.19	0.6669	0.000
ENJMATH1	-0.12568358	0.12332196	1.04	0.3081	0.000
REPEAT1	0.22180638	0.30722097	0.52	0.4703	0.000
GRADE1	0.16187109	0.18686182	0.75	0.3863	0.000
ABSMUCH1	1.25085944	0.29435441	18.06	0.0000	0.166
EPOSTHS1	0.27910155	0.30036456	0.86	0.3528	0.000
FAMSIZE1	0.04537581	0.05924982	0.59	0.4438	0.000
BRTHORDR	-0.02572120	0.08366882	0.09	0.7585	0.000
LANGPRB1	-0.82242533	0.42974644	3.66	0.0557	-0.053
BLACK	-0.05747876	0.42462129	0.02	0.8923	0.000
HISPANIC	0.19964268	0.46438354	0.18	0.6673	0.000
ASIAN	0.10173629	0.55194749	0.03	0.8538	0.000
BOSTON	1.01815693	0.60622407	2.82	0.0931	0.037
FRESNO	0.22914278	0.65637760	0.12	0.7270	0.000
PORTLAND	-0.37782206	0.77795418	0.24	0.6272	0.000
SANDIEGO	2.75002324	0.55041151	24.96	0.0000	0.198

Table D.31

LOGIT ANALYSIS OF THE RESTRICTIVE MEASURE OF DROPOUT AFTER SY 1986-87 WITH SITE-TREATMENT INTERACTION TERMS FOR COHORT II YOUTH (ALL SITES INCLUDED)

```
DEPENDENT VARIABLE: DROPOUT

1393 OBSERVATIONS
1318  DROPOUT =  0
  75  DROPOUT =  1
 240 OBSERVATIONS DELETED DUE TO MISSING VALUES

VARIABLE          BETA        STD. ERROR      CHI-SQUARE       P          R

INTERCEPT    -5.09421849      2.60870763         3.81       0.0508
BOS_T        -0.05134173      0.60215314         0.01       0.9321     0.000
FRESNO_T     -1.05289423      0.83720883         1.58       0.2085     0.000
PORT_T        7.77885012          .               .           .          .
DIEGO_T      -0.40443207      0.33626506         1.45       0.2291     0.000
SEATTL_T      0.92157788      1.16722879         0.62       0.4298     0.000
STDREAD1     -0.00514866      0.00297026         3.00       0.0830    -0.041
STDMATH1      0.00363260      0.00234817         2.39       0.1219     0.026
MALE         -0.25596534      0.26692513         0.92       0.3376     0.000
ENJREAD1     -0.07378730      0.13782289         0.29       0.5924     0.000
ENJMATH1     -0.11705937      0.12378948         0.89       0.3443     0.000
REPEAT1       0.22174510      0.30732827         0.52       0.4706     0.000
GRADE1        0.16822466      0.18722575         0.81       0.3689     0.000
ABSMUCH1      1.22527925      0.29518606        17.23       0.0000     0.161
EPOSTHS1      0.29157984      0.30123059         0.94       0.3331     0.000
FAMSIZE1      0.04715760      0.05911830         0.64       0.4251     0.000
BRTHORDR     -0.02599903      0.08382847         0.10       0.7565     0.000
LANGPRB1     -0.82107047      0.43043885         3.64       0.0565    -0.053
BLACK        -0.02829534      0.42383814         0.00       0.9468     0.000
HISPANIC      0.20445604      0.46403588         0.19       0.6595     0.000
ASIAN         0.12593368      0.55273518         0.05       0.8198     0.000
BOSTON        1.66317593      1.10238512         2.28       0.1314     0.022
FRESNO        1.24961703      1.11741172         1.25       0.2634     0.000
PORTLAND     -6.95572447          .               .           .          .
SANDIEGO      3.55234527      1.04031238        11.66       0.0006     0.129
```

Table D.32

LOGIT ANALYSIS OF THE RESTRICTIVE MEASURE OF DROPOUT AFTER SY 1986-87 WITH RACIAL/ETHNIC-TREATMENT INTERACTION TERMS FOR COHORT II YOUTH (ALL SITES INCLUDED)

DEPENDENT VARIABLE: DROPOUT

1393 OBSERVATIONS
1318 DROPOUT = 0
75 DROPOUT = 1
240 OBSERVATIONS DELETED DUE TO MISSING VALUES

VARIABLE	BETA	STD. ERROR	CHI-SQUARE	P	R
INTERCEPT	-3.89793266	2.49038382	2.45	0.1175	
BLACK_T	-0.06069592	0.38864113	0.02	0.8759	0.000
HISP_T	-0.45497401	0.53132438	0.73	0.3918	0.000
ASIAN_T	0.30556425	0.59150644	0.27	0.6054	0.000
OTHER_T	-1.40027658	0.87119207	2.58	0.1080	-0.032
STDREAD1	-0.00548177	0.00304626	3.24	0.0719	-0.046
STDMATH1	0.00376026	0.00235760	2.54	0.1107	0.031
MALE	-0.27776190	0.26879100	1.07	0.3014	0.000
ENJREAD1	-0.08753695	0.13826125	0.40	0.5267	0.000
ENJMATH1	-0.12712572	0.12406785	1.05	0.3055	0.000
REPEAT1	0.26673866	0.30899634	0.75	0.3880	0.000
GRADE1	0.17681598	0.18903046	0.87	0.3496	0.000
ABSMUCH1	1.24070235	0.29634603	17.53	0.0000	0.163
EPOSTHS1	0.33233164	0.30281678	1.20	0.2724	0.000
FAMSIZE1	0.05581942	0.05973951	0.87	0.3501	0.000
BRTHORDR	-0.01808590	0.08388967	0.05	0.8293	0.000
LANGPRB1	-0.83153477	0.43047489	3.73	0.0534	-0.054
BLACK	-0.53220489	0.51773114	1.06	0.3040	0.000
HISPANIC	-0.09418643	0.54950646	0.03	0.8639	0.000
ASIAN	-0.57499745	0.68527373	0.70	0.4014	0.000
BOSTON	1.00701887	0.60860193	2.74	0.0980	0.036
FRESNO	0.17443491	0.65859723	0.07	0.7911	0.000
PORTLAND	-0.37703750	0.77906499	0.23	0.6284	0.000
SANDIEGO	2.73748028	0.55252422	24.55	0.0000	0.196

Table D.33

LOGIT ANALYSIS OF THE RESTRICTIVE MEASURE OF DROPOUT AFTER SY 1986-87 WITH GENDER-TREATMENT INTERACTION TERMS FOR COHORT II YOUTH (ALL SITES INCLUDED)

```
DEPENDENT VARIABLE: DROPOUT

1393 OBSERVATIONS
1318  DROPOUT =  0
  75  DROPOUT =  1
 240 OBSERVATIONS DELETED DUE TO MISSING VALUES

VARIABLE          BETA         STD. ERROR       CHI-SQUARE       P        R

INTERCEPT    -4.34301284      2.45867566          3.12      0.0773
MALE_T       -0.31350521      0.39430201          0.63      0.4266    0.000
FEMALE_T     -0.15875731      0.33988778          0.22      0.6404    0.000
STDREAD1     -0.00536268      0.00301108          3.17      0.0749   -0.045
STDMATH1      0.00383267      0.00235084          2.66      0.1030    0.034
MALE         -0.18210009      0.35539137          0.26      0.6084    0.000
ENJREAD1     -0.05818098      0.13741386          0.18      0.6720    0.000
ENJMATH1     -0.12369750      0.12343424          1.00      0.3163    0.000
REPEAT1       0.21958971      0.30731873          0.51      0.4749    0.000
GRADE1        0.16249793      0.18700207          0.76      0.3849    0.000
ABSMUCH1      1.24732886      0.29458027         17.93      0.0000    0.165
EPOSTHS1      0.28205824      0.30057467          0.88      0.3480    0.000
FAMSIZE1      0.04498350      0.05923454          0.58      0.4476    0.000
BRTHORDR     -0.02500686      0.08369221          0.09      0.7651    0.000
LANGPRB1     -0.82390011      0.43013278          3.67      0.0554   -0.053
BLACK        -0.06541081      0.42560310          0.02      0.8779    0.000
HISPANIC      0.19323207      0.46478212          0.17      0.6776    0.000
ASIAN         0.10224608      0.55222416          0.03      0.8531    0.000
BOSTON        1.02166250      0.60641544          2.84      0.0920    0.038
FRESNO        0.23307753      0.65663569          0.13      0.7226    0.000
PORTLAND     -0.37511388      0.77805366          0.23      0.6297    0.000
SANDIEGO      2.75450436      0.55066006         25.02      0.0000    0.199
```

Table D.34

LOGIT ANALYSIS OF THE LESS RESTRICTIVE MEASURE OF DROPOUT AFTER SY 1986-87 FOR COHORT II YOUTH (ALL SITES INCLUDED)

```
DEPENDENT VARIABLE: DROP2

1393 OBSERVATIONS
1173  DROP2    =  0
 220  DROP2    =  1
 240 OBSERVATIONS DELETED DUE TO MISSING VALUES

VARIABLE           BETA        STD. ERROR      CHI-SQUARE         P        R

INTERCEPT   -2.40619074     1.40942393           2.91       0.0878
TREAT        0.07207106     0.15345848           0.22       0.6386    0.000
STDREAD1    -0.00073978     0.00167589           0.19       0.6589    0.000
STDMATH1     0.00031646     0.00139207           0.05       0.8202    0.000
MALE         0.05387869     0.15871924           0.12       0.7343    0.000
ENJREAD1    -0.01735615     0.07960860           0.05       0.8274    0.000
ENJMATH1    -0.10761530     0.07202357           2.23       0.1351   -0.014
REPEAT1      0.05329238     0.18163770           0.09       0.7692    0.000
GRADE1       0.04984461     0.11617715           0.18       0.6679    0.000
ABSMUCH1     0.83245758     0.17148223          23.57       0.0000    0.133
EPOSTHS1    -0.15737958     0.17242017           0.83       0.3614    0.000
FAMSIZE1     0.03794395     0.03841494           0.98       0.3233    0.000
BRTHORDR    -0.02724704     0.04949180           0.30       0.5820    0.000
LANGPRB1    -0.49230336     0.26700114           3.40       0.0652   -0.034
BLACK       -0.16732350     0.22160698           0.57       0.4502    0.000
HISPANIC     0.10427064     0.26608353           0.15       0.6952    0.000
ASIAN       -0.09143706     0.32330765           0.08       0.7773    0.000
BOSTON       0.18345598     0.32912295           0.31       0.5772    0.000
FRESNO       0.80721961     0.30840287           6.85       0.0089    0.063
PORTLAND     1.15152296     0.28970424          15.80       0.0001    0.107
SANDIEGO     1.57014731     0.29004626          29.31       0.0000    0.150
```

Table D.35

LOGIT ANALYSIS OF THE LESS RESTRICTIVE MEASURE OF DROPOUT AFTER SY 1986-87 WITH SITE-TREATMENT INTERACTION TERMS FOR COHORT II YOUTH (ALL SITES INCLUDED)

```
DEPENDENT VARIABLE: DROP2

1393 OBSERVATIONS
1173  DROP2    =  0
 220  DROP2    =  1
 240 OBSERVATIONS DELETED DUE TO MISSING VALUES

VARIABLE          BETA          STD. ERROR       CHI-SQUARE        P          R

INTERCEPT   -2.39439194      1.43699275         2.78         0.0957
BOS_T       -0.07041669      0.41734778         0.03         0.8660     0.000
FRESNO_T     0.17949550      0.31628501         0.32         0.5704     0.000
PORT_T       0.66888171      0.32439850         4.25         0.0392     0.043
DIEGO_T     -0.40500156      0.28835830         1.97         0.1602     0.000
SEATTL_T    -0.02002597      0.48205755         0.00         0.9669     0.000
STDREAD1    -0.00075636      0.00167967         0.20         0.6525     0.000
STDMATH1     0.00030003      0.00140237         0.05         0.8306     0.000
MALE         0.06199655      0.15938234         0.15         0.6973     0.000
ENJREAD1    -0.01079898      0.08001758         0.02         0.8926     0.000
ENJMATH1    -0.10857605      0.07243351         2.25         0.1339    -0.014
REPEAT1      0.04991029      0.18187037         0.08         0.7838     0.000
GRADE1       0.05168462      0.11658220         0.20         0.6575     0.000
ABSMUCH1     0.82532376      0.17217807        22.98         0.0000     0.131
EPOSTHS1    -0.15090333      0.17343891         0.76         0.3843     0.000
FAMSIZE1     0.04096674      0.03852591         1.13         0.2876     0.000
BRTHORDR    -0.02731898      0.04978853         0.30         0.5832     0.000
LANGPRB1    -0.47540003      0.26795861         3.15         0.0760    -0.031
BLACK       -0.15561423      0.22275593         0.49         0.4848     0.000
HISPANIC     0.10713387      0.26705893         0.16         0.6883     0.000
ASIAN       -0.09673570      0.32469176         0.09         0.7658     0.000
BOSTON       0.20948528      0.46390318         0.20         0.6516     0.000
FRESNO       0.70585408      0.43399912         2.65         0.1039     0.023
PORTLAND     0.75206873      0.43668513         2.97         0.0850     0.028
SANDIEGO     1.74518052      0.40580978        18.49         0.0000     0.116
```

Table D.36

LOGIT ANALYSIS OF THE LESS RESTRICTIVE MEASURE OF DROPOUT AFTER SY 1986-87 WITH RACIAL/ETHNIC-TREATMENT INTERACTION TERMS FOR COHORT II YOUTH (ALL SITES INCLUDED)

DEPENDENT VARIABLE: DROP2

1393 OBSERVATIONS
1173 DROP2 = 0
220 DROP2 = 1
240 OBSERVATIONS DELETED DUE TO MISSING VALUES

VARIABLE	BETA	STD. ERROR	CHI-SQUARE	P	R
INTERCEPT	-2.45177236	1.42224423	2.97	0.0847	
BLACK_T	0.03421561	0.23310860	0.02	0.8833	0.000
HISP_T	-0.08308559	0.32134182	0.07	0.7960	0.000
ASIAN_T	0.39161900	0.40488148	0.94	0.3334	0.000
OTHER_T	0.09614775	0.35801679	0.07	0.7883	0.000
STDREAD1	-0.00065237	0.00168205	0.15	0.6981	0.000
STDMATH1	0.00024035	0.00139726	0.03	0.8634	0.000
MALE	0.04095742	0.15954671	0.07	0.7974	0.000
ENJREAD1	-0.02264488	0.07992813	0.08	0.7769	0.000
ENJMATH1	-0.10600679	0.07226420	2.15	0.1424	-0.011
REPEAT1	0.05702864	0.18172913	0.10	0.7537	0.000
GRADE1	0.05380101	0.11648008	0.21	0.6442	0.000
ABSMUCH1	0.82375542	0.17226300	22.87	0.0000	0.131
EPOSTHS1	-0.15774697	0.17271148	0.83	0.3611	0.000
FAMSIZE1	0.04008211	0.03850543	1.08	0.2979	0.000
BRTHORDR	-0.02726326	0.04956308	0.30	0.5823	0.000
LANGPRB1	-0.47481749	0.26791824	3.14	0.0764	-0.031
BLACK	-0.13559263	0.31053728	0.19	0.6624	0.000
HISPANIC	0.19045128	0.35389566	0.29	0.5905	0.000
ASIAN	-0.26905568	0.44336662	0.37	0.5440	0.000
BOSTON	0.18649500	0.32927443	0.32	0.5711	0.000
FRESNO	0.80562743	0.30904210	6.80	0.0091	0.063
PORTLAND	1.15142404	0.28979242	15.79	0.0001	0.107
SANDIEGO	1.56673789	0.29064541	29.06	0.0000	0.149

Table D.37

LOGIT ANALYSIS OF THE LESS RESTRICTIVE MEASURE OF DROPOUT AFTER SY 1986-87 WITH GENDER-TREATMENT INTERACTION TERMS FOR COHORT II YOUTH (ALL SITES INCLUDED)

DEPENDENT VARIABLE: DROP2

1393 OBSERVATIONS
1173 DROP2 = 0
220 DROP2 = 1
240 OBSERVATIONS DELETED DUE TO MISSING VALUES

VARIABLE	BETA	STD. ERROR	CHI-SQUARE	P	R
INTERCEPT	-2.45386590	1.41380281	3.01	0.0826	
MALE_T	-0.01962599	0.21949755	0.01	0.9288	0.000
FEMALE_T	0.16071757	0.21606909	0.55	0.4570	0.000
STDREAD1	-0.00070804	0.00167886	0.18	0.6732	0.000
STDMATH1	0.00025136	0.00139673	0.03	0.8572	0.000
MALE	0.14710065	0.22515523	0.43	0.5135	0.000
ENJREAD1	-0.01768023	0.07960965	0.05	0.8242	0.000
ENJMATH1	-0.10599315	0.07203621	2.16	0.1412	-0.012
REPEAT1	0.05293638	0.18163483	0.08	0.7707	0.000
GRADE1	0.05160914	0.11630068	0.20	0.6572	0.000
ABSMUCH1	0.83187918	0.17150367	23.53	0.0000	0.133
EPOSTHS1	-0.15641068	0.17247388	0.82	0.3645	0.000
FAMSIZE1	0.03849868	0.03843133	1.00	0.3165	0.000
BRTHORDR	-0.02682584	0.04949312	0.29	0.5878	0.000
LANGPRB1	-0.48904263	0.26729339	3.35	0.0673	-0.033
BLACK	-0.17652227	0.22225662	0.63	0.4271	0.000
HISPANIC	0.09260887	0.26685986	0.12	0.7286	0.000
ASIAN	-0.09336095	0.32346757	0.08	0.7729	0.000
BOSTON	0.18876727	0.32937504	0.33	0.5666	0.000
FRESNO	0.81321480	0.30870060	6.94	0.0084	0.064
PORTLAND	1.15481680	0.28984587	15.87	0.0001	0.107
SANDIEGO	1.57731495	0.29044386	29.49	0.0000	0.150

Table D.38

REGRESSION ANALYSIS OF POSTPROGRAM (FALL 1987) CONTRACEPTIVE KNOWLEDGE FOR COHORT II

```
DEP VARIABLE: AKNWLDG5
ANALYSIS OF VARIANCE

SOURCE      DF        SUM OF SQUARES      MEAN SQUARE      F VALUE      PROB>F
MODEL       25          1708.37413        68.33496505      28.757       0.0001
ERROR     1251          2972.73159         2.37628424
C TOTAL   1276          4681.10572

ROOT MSE                1.54152           R-SQUARE         0.3650
DEP MEAN                5.237275          ADJ R-SQ         0.3523
C.V.                    29.43363

PARAMETER ESTIMATES

                          PARAMETER          STANDARD       T FOR H0:
VARIABLE    DF             ESTIMATE             ERROR       PARAMETER=0     PROB > |T|

INTERCEP     1          -1.73049574        0.62545125       -2.767           0.0057
TREAT        1           1.30572347        0.08698658       15.011           0.0001
AGE1         1           0.08556924        0.08854760        0.966           0.3340
MALE         1           0.11619813        0.09484429        1.225           0.2208
ASIAN        1          -0.30036250        0.18075201       -1.662           0.0968
BLACK        1           0.18733850        0.12875886        1.455           0.1459
HISPANIC     1          -0.43066507        0.15959895       -2.698           0.0071
FAMSIZE1     1          -0.02071312        0.02384205       -0.869           0.3851
BRTHORDR     1           0.01585812        0.02704618        0.586           0.5578
LANGPRB1     1          -0.40234739        0.14463004       -2.782           0.0055
ABSMUCH1     1          -0.19436931        0.10710916       -1.815           0.0698
STDREAD1     1           0.005710019       0.000817035       6.989           0.0001
AKNWLDG1     1           0.16628778        0.02881191        5.771           0.0001
AVAILBL1     1           0.13850898        0.02714060        5.103           0.0001
KNOWCON1     1           0.16516790        0.07182990        2.299           0.0216
JSPNSBL1     1           0.37000703        0.10368646        3.569           0.0004
SEXED11      1           0.39124177        0.10489015        3.730           0.0002
SEXEDNR      1           0.48524379        0.22070784        2.199           0.0281
EVERSX11     1           0.12253574        0.10704869        1.145           0.2526
EVERSXNR     1          -0.09508388        0.18474187       -0.515           0.6069
FEMHD11      1           0.10403455        0.10056556        1.034           0.3011
FEMHDNR      1           0.20817845        0.21800473        0.955           0.3398
BOSTON       1           0.02772163        0.14477751        0.191           0.8482
FRESNO       1           0.65765836        0.14745047        4.460           0.0001
PORTLAND     1           0.55530672        0.14184371        3.915           0.0001
SANDIEGO     1           0.56362289        0.14396145        3.915           0.0001
```

Table D.39

LOGIT ANALYSIS OF THE PROBABILITY OF USING CONTRACEPTION DURING LAST INTERCOURSE FOR SEXUALLY ACTIVE COHORT II YOUTH (FALL 1987)

DEPENDENT VARIABLE: CNCONTR5

414 OBSERVATIONS
169 CNCONTR5= 0
245 CNCONTR5= 1
881 OBSERVATIONS DELETED DUE TO MISSING VALUES

VARIABLE	BETA	STD. ERROR	CHI-SQUARE	P	R
INTERCEPT	-1.96074279	1.57855725	1.54	0.2142	
TREAT	0.31167920	0.22175449	1.98	0.1599	0.000
AGE1	0.29770851	0.22403469	1.77	0.1839	0.000
BLACK	-0.35363242	0.31855643	1.23	0.2670	0.000
HISPANIC	-0.90581388	0.40406092	5.03	0.0250	-0.074
FAMSIZE1	-0.02405096	0.05767123	0.17	0.6767	0.000
BRTHORDR	0.14397044	0.07005796	4.22	0.0399	0.063
ABSMUCH1	-0.70977253	0.24251532	8.57	0.0034	-0.108
STDREAD1	0.00237174	0.00201683	1.38	0.2396	0.000
AKNWLDG1	-0.11374265	0.07102808	2.56	0.1093	-0.032
AVAILBL1	0.05502761	0.06737631	0.67	0.4141	0.000
KNOWCON1	0.09952955	0.17708565	0.32	0.5741	0.000
JSPNSBL1	0.27346190	0.26113372	1.10	0.2950	0.000
SEXED11	0.47274821	0.24753383	3.65	0.0562	0.054
EVERSX11	0.32808782	0.53895969	0.37	0.5427	0.000
FEMHD11	0.14070590	0.23732450	0.35	0.5533	0.000
CNCONT11	0.82147726	0.27830891	8.71	0.0032	0.109
CNCONTNR	-0.00242562	0.56779645	0.00	0.9966	0.000
BOSTON	-0.30877260	0.36211677	0.73	0.3938	0.000
FRESNO	0.00246438	0.38343747	0.00	0.9949	0.000
PORTLAND	0.21954522	0.36811331	0.36	0.5509	0.000
SANDIEGO	-0.68059185	0.38923118	3.06	0.0804	-0.043

Table D.40

LOGIT ANALYSIS OF THE PROBABILITY OF USING CONTRACEPTION DURING LAST INTERCOURSE WITH GENDER-TREATMENT INTERACTION TERMS FOR SEXUALLY ACTIVE COHORT II YOUTH (FALL 1987)

```
DEPENDENT VARIABLE: CNCONTR5

414 OBSERVATIONS
169   CNCONTR5=  0
245   CNCONTR5=  1
881 OBSERVATIONS DELETED DUE TO MISSING VALUES

VARIABLE          BETA        STD. ERROR       CHI-SQUARE        P        R

INTERCEPT   -2.15730898      1.59208873          1.84       0.1754
AGE1         0.31191859      0.22488106          1.92       0.1654    0.000
BLACK       -0.32253585      0.31946616          1.02       0.3127    0.000
HISPANIC    -0.87357878      0.40541671          4.64       0.0312   -0.069
FAMSIZE1    -0.02584930      0.05780906          0.20       0.6548    0.000
BRTHORDR     0.14626636      0.07015762          4.35       0.0371    0.065
ABSMUCH1    -0.67693287      0.24403483          7.69       0.0055   -0.101
STDREAD1     0.00264732      0.00203740          1.69       0.1938    0.000
AKNWLDG1    -0.12086476      0.07149213          2.86       0.0909   -0.039
AVAILBL1     0.06274476      0.06773166          0.86       0.3543    0.000
KNOWCON1     0.09527670      0.17714565          0.29       0.5907    0.000
JSPNSBL1     0.26524480      0.26187647          1.03       0.3111    0.000
SEXED11      0.49629242      0.24840323          3.99       0.0457    0.060
EVERSX11     0.26989409      0.54225142          0.25       0.6187    0.000
FEMHD11      0.14858241      0.23790574          0.39       0.5323    0.000
MALE_TRT     0.49812471      0.26658165          3.49       0.0617    0.052
FEMALE_T     0.06397455      0.29141675          0.05       0.8262    0.000
CNCONT11     0.80052798      0.27953815          8.20       0.0042    0.105
CNCONTNR     0.00690515      0.56993533          0.00       0.9903    0.000
BOSTON      -0.31630835      0.36296898          0.76       0.3835    0.000
FRESNO      -0.01947571      0.38469281          0.00       0.9596    0.000
PORTLAND     0.22602161      0.36904909          0.38       0.5402    0.000
SANDIEGO    -0.68567529      0.38962632          3.10       0.0784   -0.044
```

Table D.41

REGRESSION ANALYSIS OF SPRING 1987 READING SCORES (IN NCES) WITH SITE-TREATMENT INTERACTION TERMS FOR COHORT I YOUTH (PORTLAND EXCLUDED)

```
DEP VARIABLE: RNCE     Normal Curve Equivalent of RSCORE
ANALYSIS OF VARIANCE

SOURCE      DF      SUM OF SQUARES       MEAN SQUARE        F VALUE        PROB>F
MODEL       20      60865.06419          3043.25321         18.660         0.0001
ERROR      558      91001.90309          163.08585
C TOTAL    578      151866.97

ROOT MSE            12.77051             R-SQUARE           0.4008
DEP MEAN            45.41727             ADJ R-SQ           0.3793
C.V.                28.11817


PARAMETER ESTIMATES
                         PARAMETER          STANDARD         T FOR H0:
VARIABLE    DF           ESTIMATE           ERROR            PARAMETER=0     PROB > |T|

INTERCEP     1         -74.75375966       10.08387459         -7.413          0.0001
TREAT        1           1.58339215        2.22861023          0.710          0.4777
BOS_T        1          -3.31056949        3.06103043         -1.082          0.2799
FRESNO_T     1          -1.50609358        3.07223156         -0.490          0.6242
DIEGO_T      1           0.49780008        3.11284596          0.160          0.8730
BLACK        1          -1.45573626        2.05405353         -0.709          0.4788
HISPANIC     1          -1.96698361        2.37730590         -0.827          0.4084
ASIAN        1           1.55638467        2.67250480          0.582          0.5606
BOSTON       1          -7.28676208        2.27315806         -3.206          0.0014
FRESNO       1          -5.57532559        2.31464898         -2.409          0.0163
SANDIEGO     1          -5.30009724        2.20946923         -2.399          0.0168
STDREAD1     1           0.15989366        0.01056700         15.131          0.0001
ENJREAD1     1           0.48616984        0.53037903          0.917          0.3597
REPEAT1      1          -1.27472261        1.40505016         -0.907          0.3647
GRADE1       1           1.20313798        0.91074457          1.321          0.1870
ABSMUCH1     1          -1.61619204        1.37731750         -1.173          0.2411
EPOSTHS1     1           3.04800344        1.30300231          2.339          0.0197
FAMSIZE1     1          -0.49838824        0.27881306         -1.788          0.0744
BRTHORDR     1          -0.32242226        0.24518880         -1.315          0.1891
LANGPRB1     1           0.06255689        2.16109648          0.029          0.9769
MALE         1          -0.70139937        1.09385430         -0.641          0.526
```

Table D.42

REGRESSION ANALYSIS OF SPRING 1987 READING SCORES (IN NCES)
WITH RACIAL/ETHNIC- AND GENDER-TREATMENT INTERACTION TERMS
FOR COHORT I YOUTH
(PORTLAND EXCLUDED)

DEP VARIABLE: RNCE   Normal Curve Equivalent of RSCORE

ANALYSIS OF VARIANCE

SOURCE	DF	SUM OF SQUARES	MEAN SQUARE	F VALUE	PROB>F
MODEL	21	60806.43521	2895.54453	17.711	0.0001
ERROR	557	91060.53207	163.48390		
C TOTAL	578	151866.97			

ROOT MSE	12.78608	R-SQUARE	0.4004
DEP MEAN	45.41727	ADJ R-SQ	0.3778
C.V.	28.15247		

PARAMETER ESTIMATES

VARIABLE	DF	PARAMETER ESTIMATE	STANDARD ERROR	T FOR H0: PARAMETER=0	PROB > |T|
INTERCEP	1	-73.62612665	10.13179406	-7.267	0.0001
TREAT	1	0.65350324	3.92427867	0.167	0.8678
BLACK_T	1	-0.63816938	4.03919345	-0.158	0.8745
HISP_T	1	2.68852659	4.53594037	0.593	0.5536
ASIAN_T	1	-1.16988725	4.52738571	-0.258	0.7962
MALE_T	1	-0.26126262	2.15746988	-0.121	0.9037
BLACK	1	-1.30079160	2.75217600	-0.473	0.6367
HISPANIC	1	-3.43656508	3.14781311	-1.092	0.2754
ASIAN	1	1.98412129	3.38391775	0.586	0.5579
BOSTON	1	-8.97504688	1.69089825	-5.308	0.0001
FRESNO	1	-6.47851335	1.75962163	-3.682	0.0003
SANDIEGO	1	-5.22271517	1.62697868	-3.210	0.0014
STDREAD1	1	0.15882363	0.01050999	15.112	0.0001
ENJREAD1	1	0.49947996	0.53103957	0.941	0.3473
REPEAT1	1	-1.41291173	1.40841784	-1.003	0.3162
GRADE1	1	1.21929222	0.91369754	1.334	0.1826
ABSMUCH1	1	-1.64341806	1.38216441	-1.189	0.2349
EPOSTHS1	1	3.19768886	1.30122488	2.457	0.0143
FAMSIZE1	1	-0.50348269	0.27857666	-1.807	0.0712
BRTHORDR	1	-0.31590405	0.24573040	-1.286	0.1991
LANGPRB1	1	0.001855161	2.18174867	0.001	0.9993
MALE	1	-0.41432025	1.50989900	-0.274	0.7839

Table D.43

REGRESSION ANALYSIS OF SPRING 1987 MATH SCORES (IN NCES) WITH SITE-TREATMENT INTERACTION TERMS FOR COHORT I YOUTH (PORTLAND EXCLUDED)

```
DEP VARIABLE: MNCE     Normal Curve Equivalent of MSCORE
ANALYSIS OF VARIANCE

SOURCE      DF       SUM OF SQUARES       MEAN SQUARE        F VALUE        PROB>F
MODEL       21         187593.95          8933.04535         40.031         0.0001
ERROR      554         123626.90           223.15324
C TOTAL    575         311220.85

ROOT MSE               14.93831           R-SQUARE           0.6028
DEP MEAN               56.5191            ADJ R-SQ           0.5877
C.V.                   26.43056

PARAMETER ESTIMATES
                           PARAMETER           STANDARD        T FOR H0:
VARIABLE     DF            ESTIMATE              ERROR         PARAMETER=0      PROB > |T|

INTERCEP      1          -80.46110004        11.94229461         -6.737           0.0001
TREAT         1            2.90728800         2.60845482          1.115           0.2655
BOS_T         1            0.76694303         3.59066836          0.214           0.8309
FRESNO_T      1            0.55563027         3.61746595          0.154           0.8780
DIEGO_T       1           -2.67424174         3.64363803         -0.734           0.4633
STDMATH1      1            0.15086732         0.01203185         12.539           0.0001
ENJMATH1      1            2.39944810         0.59107489          4.059           0.0001
STDREAD1      1            0.02276540         0.01515793          1.502           0.1337
REPEAT1       1           -2.58593249         1.66158349         -1.556           0.1202
GRADE1        1            0.11678981         1.07827324          0.108           0.9138
ABSMUCH1      1           -1.53252265         1.61243844         -0.950           0.3423
EPOSTHS1      1            3.92096539         1.53264042          2.558           0.0108
FAMSIZE1      1            0.006686639        0.32915496          0.020           0.9838
BRTHORDR      1            0.09968664         0.29586790          0.337           0.7363
LANGPRB1      1            2.31709505         2.52622065          0.917           0.3594
MALE          1            1.37031198         1.28730217          1.064           0.2876
BLACK         1            0.22700197         2.42131803          0.094           0.9253
HISPANIC      1            0.85647220         2.81364060          0.304           0.7609
ASIAN         1           14.30190654         3.20746660          4.459           0.0001
BOSTON        1           -8.39663666         2.69138883         -3.120           0.0019
FRESNO        1           -4.87949343         2.76904407         -1.762           0.0786
SANDIEGO      1            1.98140031         2.62305374          0.755           0.4503
```

Table D.44

REGRESSION ANALYSIS OF SPRING 1987 MATH SCORES (IN NCES) WITH RACIAL/ETHNIC- AND GENDER-TREATMENT INTERACTION TERMS FOR COHORT I YOUTH (PORTLAND EXCLUDED)

```
DEP VARIABLE: MNCE     Normal Curve Equivalent of MSCORE
ANALYSIS OF VARIANCE

SOURCE      DF      SUM OF SQUARES      MEAN SQUARE      F VALUE      PROB>F
MODEL       22         189517.91        8614.45043       39.143       0.0001
ERROR      553         121702.94        220.07765
C TOTAL    575         311220.85

ROOT MSE                14.83501        R-SQUARE         0.6089
DEP MEAN                56.5191         ADJ R-SQ         0.5934
C.V.                    26.24779


PARAMETER ESTIMATES
                          PARAMETER          STANDARD       T FOR H0:
VARIABLE    DF             ESTIMATE             ERROR     PARAMETER=0    PROB > |T|

INTERCEP     1         -76.44004454       11.90810341          -6.419        0.0001
TREAT        1         -10.14291146        4.61132334          -2.200        0.0283
BLACK_T      1          14.74091415        4.74796151           3.105        0.0020
HISP_T       1          13.41878307        5.33565087           2.515        0.0122
ASIAN_T      1          11.32435568        5.31754147           2.130        0.0336
MALE_T       1           0.09603138        2.51421889           0.038        0.9695
STDMATH1     1           0.15007918        0.01191611          12.595        0.0001
ENJMATH1     1           2.41551008        0.58697761           4.115        0.0001
STDREAD1     1           0.02424036        0.01489262           1.628        0.1042
REPEAT1      1          -2.30777516        1.64874330          -1.400        0.1622
GRADE1       1           0.24700134        1.07226646           0.230        0.8179
ABSMUCH1     1          -1.28664042        1.60572598          -0.801        0.4233
EPOSTHS1     1           3.94178410        1.51787122           2.597        0.0097
FAMSIZE1     1           0.01110975        0.32580715           0.034        0.9728
BRTHORDR     1           0.09176571        0.29399756           0.312        0.7551
LANGPRB1     1           2.34420767        2.52661079           0.928        0.3539
MALE         1           1.36566018        1.78525373           0.765        0.4446
BLACK        1          -6.26789887        3.20399864          -1.956        0.0509
HISPANIC     1          -4.87640633        3.70775021          -1.315        0.1890
ASIAN        1           9.51005602        3.95362455           2.405        0.0165
BOSTON       1          -8.34158646        1.99048984          -4.191        0.0001
FRESNO       1          -4.92812742        2.10354321          -2.343        0.0195
SANDIEGO     1           0.67939444        1.92566523           0.353        0.7244
```

Table D.45

REGRESSION ANALYSIS OF THE PERCENTAGE OF SY 1986-87 ABSENT WITH SITE-TREATMENT INTERACTION TERMS FOR COHORT I YOUTH (SAN DIEGO EXCLUDED)

```
DEP VARIABLE: PCTABS
ANALYSIS OF VARIANCE

SOURCE      DF     SUM OF SQUARES     MEAN SQUARE     F VALUE     PROB>F

MODEL       22       2.29885193       0.10449327        5.313     0.0001
ERROR      796      15.65395837       0.01966578
C TOTAL    818      17.95281030

ROOT MSE             0.1402347        R-SQUARE       0.1280
DEP MEAN             0.1476266        ADJ R-SQ       0.1040
C.V.                 94.99287


PARAMETER ESTIMATES
                       PARAMETER          STANDARD       T FOR H0:
VARIABLE    DF          ESTIMATE             ERROR     PARAMETER=0     PROB > |T|

INTERCEP     1        0.24027670        0.09009284           2.667         0.0078
TREAT        1     -0.000808089         0.01934250          -0.042         0.9667
BOS_T        1      -0.01113578         0.02793040          -0.399         0.6902
FRESNO_T     1       0.02289981         0.02758694           0.830         0.4067
PORT_T       1     -0.005388618         0.02805937          -0.192         0.8478
STDREAD1     1     -0.000195106        0.000118138          -1.652         0.0990
STDMATH1     1     -0.000077752        0.000091459          -0.850         0.3955
ENJREAD1     1     -0.004692852        0.005044879          -0.930         0.3525
ENJMATH1     1      0.004304593        0.004659811           0.924         0.3559
REPEAT1      1      0.001961015         0.01256126           0.156         0.8760
GRADE1       1      0.008557864        0.007767557           1.102         0.2709
ABSMUCH1     1       0.05941806         0.01160457           5.120         0.0001
EPOSTHS1     1     -0.009270789         0.01188598          -0.780         0.4356
FAMSIZE1     1     -0.007028659        0.002531552          -2.776         0.0056
BRTHORDR     1      0.005079914        0.002155145           2.357         0.0187
LANGPRB1     1      -0.03779602         0.02274281          -1.662         0.0969
MALE         1      -0.01001280         0.01007928          -0.993         0.3208
BLACK        1       0.03550646         0.01685287           2.107         0.0354
HISPANIC     1       0.04535852         0.02174364           2.086         0.0373
ASIAN        1      -0.02225587         0.02591725          -0.859         0.3907
BOSTON       1       0.06276033         0.02119795           2.961         0.0032
FRESNO       1      -0.02387460         0.02162562          -1.104         0.2699
PORTLAND     1       0.05007715         0.02040996           2.454         0.0144
```

Table D.46

REGRESSION ANALYSIS OF THE PERCENT OF SY 1986-87 ABSENT WITH RACIAL/ETHNIC- AND GENDER-TREATMENT INTERACTION TERMS FOR COHORT I YOUTH (SAN DIEGO EXCLUDED)

```
DEP VARIABLE: PCTABS
ANALYSIS OF VARIANCE

SOURCE     DF        SUM OF SQUARES       MEAN SQUARE        F VALUE       PROB>F
MODEL      23            2.29516018        0.09978957          5.067       0.0001
ERROR     795           15.65765011        0.01969516
C TOTAL   818           17.95281030

ROOT MSE                  0.1403394       R-SQUARE            0.1278
DEP MEAN                  0.1476266       ADJ R-SQ            0.1026
C.V.                       95.06381

PARAMETER ESTIMATES
                           PARAMETER          STANDARD         T FOR H0:
VARIABLE     DF             ESTIMATE             ERROR       PARAMETER=0     PROB > |T|

INTERCEP      1           0.25058885        0.09035613             2.773         0.0057
TREAT         1          -0.02485581        0.03267296            -0.761         0.4470
BLACK_T       1           0.02973492        0.03316894             0.896         0.3703
HISP_T        1           0.03167394        0.04012131             0.789         0.4301
ASIAN_T       1           0.001036496       0.04286346             0.024         0.9807
MALE_T        1           0.004037193       0.01989893             0.203         0.8393
STDREAD1      1          -0.000200947       0.000117578           -1.709         0.0878
STDMATH1      1          -0.000071672       0.000091560           -0.783         0.4340
ENJREAD1      1          -0.004805118       0.005043914           -0.953         0.3411
ENJMATH1      1           0.004453987       0.004660645            0.956         0.3395
REPEAT1       1           0.001333600       0.01257251             0.106         0.9156
GRADE1        1           0.008504914       0.007766446            1.095         0.2738
ABSMUCH1      1           0.05930559        0.01163568             5.097         0.0001
EPOSTHS1      1          -0.009279857       0.01186850            -0.782         0.4345
FAMSIZE1      1          -0.006878320       0.002527156           -2.722         0.0066
BRTHORDR      1           0.005125571       0.002155145            2.378         0.0176
LANGPRB1      1          -0.03558521        0.02290222            -1.554         0.1206
MALE          1          -0.01173071        0.01439528            -0.815         0.4154
BLACK         1           0.02223238        0.02202199             1.010         0.3130
HISPANIC      1           0.03079895        0.02865908             1.075         0.2829
ASIAN         1          -0.02264315        0.03216409            -0.704         0.4816
BOSTON        1           0.05669899        0.01578073             3.593         0.0003
FRESNO        1          -0.01249880        0.01650978            -0.757         0.4492
PORTLAND      1           0.04640781        0.01449875             3.201         0.0014
```

Table D.47

REGRESSION ANALYSIS OF THE PERCENTAGE OF HIGH SCHOOL CREDITS EARNED IN SY 1986-87 WITH SITE-TREATMENT INTERACTION TERMS FOR COHORT I YOUTH (ALL SITES INCLUDED)

```
DEP VARIABLE: PCTCRED
ANALYSIS OF VARIANCE

SOURCE     DF      SUM OF SQUARES     MEAN SQUARE      F VALUE      PROB>F
MODEL      35        27.46327991       0.78466514       53.212       0.0001
ERROR     952        14.03827111       0.01474608
C TOTAL   987        41.50155102

ROOT MSE             0.1214335        R-SQUARE         0.6617
DEP MEAN             0.2630577        ADJ R-SQ         0.6493
C.V.                 46.16229

PARAMETER ESTIMATES
                        PARAMETER        STANDARD        T FOR H0:
VARIABLE     DF          ESTIMATE           ERROR      PARAMETER=0     PROB > |T|

INTERCEP      1       -0.17681692      0.08648947          -2.044         0.0412
TREAT         1       -0.01433563      0.01674856          -0.856         0.3923
BOS_T         1        0.02301616      0.02446270           0.941         0.3470
FRESNO_T      1       -0.01350344      0.02393858          -0.564         0.5728
PORT_T        1        0.01294951      0.02440666           0.531         0.5958
DIEGO_T       1       -0.03978692      0.02509843          -1.585         0.1132
FRESNO        1       -0.004755452     0.01886109          -0.252         0.8010
PORTLAND      1        0.01026040      0.01774936           0.578         0.5634
SANDIEGO      1       -0.44906477      0.14833787          -3.027         0.0025
STRD1_CM      1         .00000750339   0.000128033          0.059         0.9533
EJRD1_CM      1        0.003577170     0.006271602          0.570         0.5686
RPT1_CM       1        0.01064571      0.01547158           0.688         0.4916
GRD1_CM       1        0.13624518      0.01104114          12.340         0.0001
ABS1_CM       1       -0.07757380      0.01806833          -4.293         0.0001
EPHS1_CM      1        0.03189287      0.01452197           2.196         0.0283
FMSZ1_CM      1        0.006282920     0.003551144          1.769         0.0772
BTODR_CM      1       -0.001084012     0.003036785         -0.357         0.7212
LGPB1_CM      1        0.01072161      0.02663705           0.403         0.6874
MALE_CM       1       -0.01813479      0.01331306          -1.362         0.1735
STRD1_AN      1        0.000080605     0.000095958          0.840         0.4011
EJRD1_AN      1       -0.007770713     0.005000926         -1.554         0.1206
RPT1_AN       1       -0.003315146     0.01296335          -0.256         0.7982
GRD1_AN       1        0.03764791      0.007284624          5.168         0.0001
ABS1_AN       1       -0.03491580      0.01119557          -3.119         0.0019
EPHS1_AN      1        0.006426377     0.01233075           0.521         0.6024
FMSZ1_AN      1        0.004181017     0.002411069          1.734         0.0832
BTODR_AN      1        1.38375E-07     0.002069029          0.000         0.9999
LGPB1_AN      1        0.01644348      0.02105435           0.781         0.4350
MALE_AN       1       -0.01095944      0.01006712          -1.089         0.2766
BLCK_CM       1       -0.03265523      0.02287834          -1.427         0.1538
HISP_CM       1       -0.000579997     0.02793384          -0.021         0.9834
ASIAN_CM      1        0.07060116      0.03258107           2.167         0.0305
BLCK_AN       1       -0.005843237     0.01834033          -0.319         0.7501
HISP_AN       1        0.01366099      0.02280311           0.599         0.5493
ASIAN_AN      1        0.01105307      0.02477435           0.446         0.6556
```

Table D.48

REGRESSION ANALYSIS OF THE PERCENTAGE OF HIGH SCHOOL CREDITS EARNED IN SY 1986-87 WITH RACIAL/ETHNIC- AND GENDER-TREATMENT INTERACTION TERMS FOR COHORT I YOUTH
(ALL SITES INCLUDED)

DEP VARIABLE: PCTCRED
ANALYSIS OF VARIANCE

SOURCE	DF	SUM OF SQUARES	MEAN SQUARE	F VALUE	PROB>F
MODEL	31	22.47128495	0.72488016	36.415	0.0001
ERROR	956	19.03026607	0.01990614		
C TOTAL	987	41.50155102			

ROOT MSE	0.1410891	R-SQUARE	0.5415
DEP MEAN	0.2630577	ADJ R-SQ	0.5266
C.V.	53.63428		

PARAMETER ESTIMATES

VARIABLE	DF	PARAMETER ESTIMATE	STANDARD ERROR	T FOR H0: PARAMETER=0	PROB > |T|
INTERCEP	1	-0.51658880	0.08086626	-6.388	0.0001
TREAT	1	-0.03286818	0.03194628	-1.029	0.3038
BLACK_T	1	0.006784632	0.03269993	0.207	0.8357
HISP_T	1	0.02720058	0.03821458	0.712	0.4768
ASIAN_T	1	0.006554916	0.03819716	0.172	0.8638
MALE_T	1	0.007209913	0.01820921	0.396	0.6922
STRD1_CM	1	-0.000635574	0.000129440	-4.910	0.0001
EJRD1_CM	1	0.01146522	0.007260371	1.579	0.1146
RPT1_CM	1	-0.01989134	0.01725811	1.153	0.2494
GRD1_CM	1	0.15690636	0.01083249	14.485	0.0001
ABS1_CM	1	-0.07611343	0.02101257	-3.622	0.0003
EPHS1_CM	1	0.03679983	0.01679699	2.191	0.0287
FMSZ1_CM	1	-0.000083794	0.004065841	-0.021	0.9836
BTODR_CM	1	-0.002532587	0.003525021	-0.718	0.4726
LGPB1_CM	1	-0.03488247	0.03076744	-1.134	0.2572
MALE_CM	1	-0.04142693	0.01770679	-2.340	0.0195
STRD1_AN	1	0.000336983	0.000102927	3.274	0.0011
EJRD1_AN	1	-0.004426221	0.005769666	-0.767	0.4432
RPT1_AN	1	0.01518661	0.01459971	1.040	0.2985
GRD1_AN	1	0.05398404	0.007891253	6.841	0.0001
ABS1_AN	1	-0.03638453	0.01301090	-2.796	0.0053
EPHS1_AN	1	0.004013951	0.01427899	0.281	0.7787
FMSZ1_AN	1	0.004510019	0.002786464	1.619	0.1059
BTODR_AN	1	0.000679092	0.002396510	0.283	0.7770
LGPB1_AN	1	0.02721105	0.02447353	1.112	0.2665
MALE_AN	1	-0.007402787	0.01493356	-0.496	0.6202
BLCK_CM	1	-0.001269910	0.03078261	-0.041	0.9671
HISP_CM	1	0.08698808	0.03653276	2.381	0.0175
ASIAN_CM	1	0.27845738	0.03902329	7.136	0.0001
BLCK_AN	1	-0.000280805	0.02480867	-0.011	0.9910
HISP_AN	1	-0.005217210	0.02975222	-0.175	0.8608
ASIAN_AN	1	0.01698374	0.03223995	0.527	0.5985

Table D.49

LOGIT ANALYSIS OF THE PROBABILITY OF PROMOTION AFTER SY 1986-87 FOR COHORT I YOUTH (BOSTON AND SEATTLE)

DEPENDENT VARIABLE: PROMOTE

424 OBSERVATIONS
 69 PROMOTE = 0
355 PROMOTE = 1
232 OBSERVATIONS DELETED DUE TO MISSING VALUES

VARIABLE	BETA	STD. ERROR	CHI-SQUARE	P	R
INTERCEPT	-5.54357547	3.13629945	3.12	0.0771	
TREAT	0.62778607	0.31796720	3.90	0.0483	0.071
STDREAD1	-0.00189344	0.00367133	0.27	0.6060	0.000
STDMATH1	0.00345889	0.00289723	1.43	0.2325	0.000
ENJREAD1	-0.04068784	0.14430693	0.08	0.7780	0.000
ENJMATH1	0.10286755	0.13301166	0.60	0.4393	0.000
REPEAT1	-0.06555139	0.35607324	0.03	0.8539	0.000
GRADE1	0.91323890	0.32144657	8.07	0.0045	0.127
ABSMUCH1	-1.09609708	0.36417347	9.06	0.0026	-0.137
EPOSTHS1	0.06394038	0.33002390	0.04	0.8464	0.000
FAMSIZE1	0.18655684	0.09788558	3.63	0.0567	0.066
BRTHORDR	-0.01236021	0.07828973	0.02	0.8746	0.000
LANGPRB1	0.59147214	0.98714769	0.36	0.5491	0.000
MALE	0.13207503	0.31206930	0.18	0.6721	0.000
BLACK	-0.61212668	0.44372297	1.90	0.1677	0.000
HISPANIC	-0.57111133	0.65230103	0.77	0.3813	0.000
BOSTON	-2.72010964	0.48967897	30.86	0.0000	-0.277

Table D.50

LOGIT ANALYSIS OF THE PROBABILITY OF PROMOTION AFTER SY 1986-87 WITH SITE-TREATMENT INTERACTION TERMS FOR COHORT I YOUTH (BOSTON AND SEATTLE)

DEPENDENT VARIABLE: PROMOTE

424 OBSERVATIONS
69 PROMOTE = 0
355 PROMOTE = 1
232 OBSERVATIONS DELETED DUE TO MISSING VALUES

VARIABLE	BETA	STD. ERROR	CHI-SQUARE	P	R
INTERCEPT	-5.76205396	3.14706504	3.35	0.0671	
STDREAD1	-0.00175440	0.00366428	0.23	0.6321	0.000
STDMATH1	0.00337451	0.00289708	1.36	0.2441	0.000
ENJREAD1	-0.03474780	0.14415409	0.06	0.8095	0.000
ENJMATH1	0.10346608	0.13240141	0.61	0.4345	0.000
REPEAT1	-0.04102375	0.35752872	0.01	0.9086	0.000
GRADE1	0.90205687	0.32079492	7.91	0.0049	0.125
ABSMUCH1	-1.10587184	0.36510309	9.17	0.0025	-0.138
EPOSTHS1	0.03665681	0.33004795	0.01	0.9116	0.000
FAMSIZE1	0.18419746	0.09739902	3.58	0.0586	0.065
BRTHORDR	-0.01053508	0.07803987	0.02	0.8926	0.000
LANGPRB1	0.59974151	0.98634486	0.37	0.5432	0.000
MALE	0.11384998	0.31236226	0.13	0.7155	0.000
BLACK	-0.60173119	0.44317917	1.84	0.1745	0.000
HISPANIC	-0.56297383	0.64964533	0.75	0.3862	0.000
BOSTON	-2.40067066	0.55374523	18.80	0.0000	-0.211
BOS_T	0.50569651	0.33886862	2.23	0.1356	0.025
SEAT_T	1.64210286	1.12129018	2.14	0.1431	0.020

Table D.51

LOGIT ANALYSIS OF THE PROBABILITY OF PROMOTION AFTER SY 1986-87 WITH RACIAL/ETHNIC-TREATMENT INTERACTION TERMS FOR COHORT I YOUTH (BOSTON AND SEATTLE)

```
DEPENDENT VARIABLE: PROMOTE

424 OBSERVATIONS
 69  PROMOTE =  0
355  PROMOTE =  1
232 OBSERVATIONS DELETED DUE TO MISSING VALUES


VARIABLE          BETA        STD. ERROR       CHI-SQUARE         P        R

INTERCEPT    -5.80225483      3.17055456          3.35        0.0672
STDREAD1     -0.00181055      0.00365145          0.25        0.6200    0.000
STDMATH1      0.00368494      0.00288045          1.64        0.2008    0.000
ENJREAD1     -0.05131997      0.14491200          0.13        0.7232    0.000
ENJMATH1      0.10410270      0.13469791          0.60        0.4396    0.000
REPEAT1      -0.10362301      0.36287198          0.08        0.7752    0.000
GRADE1        0.88486856      0.32472490          7.43        0.0064    0.120
ABSMUCH1     -1.08845308      0.36692890          8.80        0.0030   -0.134
EPOSTHS1      0.03348248      0.33317085          0.01        0.9200    0.000
FAMSIZE1      0.18759774      0.09774269          3.68        0.0549    0.067
BRTHORDR     -0.01234562      0.07825867          0.02        0.8747    0.000
LANGPRB1      0.57280439      0.99464294          0.33        0.5647    0.000
MALE          0.10674840      0.31473655          0.12        0.7345    0.000
BLACK        -0.15158750      0.54841204          0.08        0.7822    0.000
HISPANIC     -0.28171064      0.76688942          0.13        0.7134    0.000
BOSTON       -2.72481391      0.49172669         30.71        0.0000   -0.276
BLACK_T       0.42062397      0.36590019          1.32        0.2503    0.000
HISP_T        0.78614641      1.02875578          0.58        0.4448    0.000
OTHER_T       1.67312859      0.90847310          3.39        0.0655    0.061
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Table D.52

LOGIT ANALYSIS OF THE PROBABILITY OF PROMOTION AFTER SY 1986-87 WITH GENDER-TREATMENT INTERACTION TERMS FOR COHORT I YOUTH (BOSTON AND SEATTLE)

DEPENDENT VARIABLE: PROMOTE

424 OBSERVATIONS
 69 PROMOTE = 0
355 PROMOTE = 1
232 OBSERVATIONS DELETED DUE TO MISSING VALUES

VARIABLE	BETA	STD. ERROR	CHI-SQUARE	P	R
INTERCEPT	-5.53842687	3.13830224	3.11	0.0776	-
STDREAD1	-0.00188699	0.00367474	0.26	0.6076	0.000
STDMATH1	0.00344199	0.00291824	1.39	0.2382	0.000
ENJREAD1	-0.04080553	0.14435564	0.08	0.7774	0.000
ENJMATH1	0.10294506	0.13304245	0.60	0.4391	0.000
REPEAT1	-0.06679337	0.35701656	0.04	0.8516	0.000
GRADE1	0.91280552	0.32157551	8.06	0.0045	0.127
ABSMUCH1	-1.09538029	0.36448055	9.03	0.0027	-0.137
EPOSTHS1	0.06380474	0.33002042	0.04	0.8467	0.000
FAMSIZE1	0.18648348	0.09786783	3.63	0.0567	0.066
BRTHORDR	-0.01252255	0.07833979	0.03	0.8730	0.000
LANGPRB1	0.59242300	0.98743483	0.36	0.5485	0.000
MALE	0.14594581	0.42569227	0.12	0.7317	0.000
BLACK	-0.61358778	0.44483233	1.90	0.1678	0.000
HISPANIC	-0.57221601	0.65288381	0.77	0.3808	0.000
BOSTON	-2.72017252	0.48960821	30.87	0.0000	-0.277
MALE_T	0.61276953	0.44631467	1.89	0.1698	0.000
FEMALE_T	0.64238606	0.44050453	2.13	0.1448	0.018

Table D.53

LOGIT ANALYSIS OF THE RESTRICTIVE MEASURE OF DROPOUT AFTER SY 1986-87 FOR COHORT I YOUTH
(ALL SITES INCLUDED)

DEPENDENT VARIABLE: DROPOUT

311 OBSERVATIONS
167 DROPOUT = 0
144 DROPOUT = 1
279 OBSERVATIONS DELETED DUE TO MISSING VALUES

VARIABLE	BETA	STD. ERROR	CHI-SQUARE	P	R
INTERCEPT	-0.34619382	1.81119805	0.04	0.8484	
TREAT	-0.18097666	0.19815376	0.83	0.3611	0.000
STDREAD1	-0.00447096	0.00229862	3.78	0.0518	-0.044
STDMATH1	-0.00010033	0.00187557	0.00	0.9573	0.000
MALE	0.14977736	0.20123993	0.55	0.4567	0.000
ENJREAD1	-0.05428279	0.09933893	0.30	0.5848	0.000
ENJMATH1	-0.05632749	0.09253053	0.37	0.5427	0.000
REPEAT1	0.37541930	0.23257419	2.61	0.1065	0.026
GRADE1	0.05380080	0.15820657	0.12	0.7338	0.000
ABSMUCH1	0.77595889	0.21586493	12.92	0.0003	0.110
EPOSTHS1	0.04347479	0.22313475	0.04	0.8455	0.000
FAMSIZE1	0.08222968	0.05148041	2.55	0.1102	0.025
BRTHORDR	0.02070928	0.04132159	0.25	0.6162	0.000
LANGPRB1	-0.00121498	0.48542761	0.00	0.9980	0.000
BLACK	-0.75947950	0.29983172	6.42	0.0113	-0.070
HISPANIC	-0.64917589	0.36281843	3.20	0.0736	-0.036
ASIAN	-2.41722430	0.57567096	17.63	0.0000	-0.131
BOSTON	1.51105245	0.40516206	13.91	0.0002	0.115
FRESNO	0.53669199	0.43937813	1.49	0.2219	0.000
PORTLAND	-1.23508269	0.68208540	3.28	0.0702	-0.038
SANDIEGO	2.99982022	0.40525458	54.79	0.0000	0.241

Table D.54

LOGIT ANALYSIS OF THE RESTRICTIVE MEASURE OF DROPOUT AFTER SY 1986-87 WITH SITE-TREATMENT INTERACTION TERMS FOR COHORT I YOUTH (ALL SITES INCLUDED)

DEPENDENT VARIABLE: DROPOUT

1311 OBSERVATIONS
1167 DROPOUT = 0
144 DROPOUT = 1

VARIABLE	BETA	STD. ERROR	CHI-SQUARE	P	R
INTERCEPT	-0.65236131	1.84521208	0.12	0.7237	
BOS_T	-0.51656304	0.36282069	2.03	0.1545	-0.005
FRESNO_T	-0.65744580	0.50211685	1.71	0.1904	0.000
PORT_T	-0.67780653	1.23649219	0.30	0.5836	0.000
DIEGO_T	0.17785309	0.30445031	0.34	0.5591	0.000
SEATTL_T	0.34024638	0.69394185	0.24	0.6239	0.000
STDREAD1	-0.00468041	0.00231243	4.10	0.0430	-0.048
STDMATH1	0.00008090	0.00188445	0.00	0.9658	0.000
MALE	0.14037895	0.20195113	0.48	0.4870	0.000
ENJREAD1	-0.04345348	0.10002444	0.19	0.6640	0.000
ENJMATH1	-0.06316578	0.09319217	0.46	0.4979	0.000
REPEAT1	0.38264918	0.23308093	2.70	0.1007	0.028
GRADE1	0.05876409	0.15907890	0.14	0.7118	0.000
ABSMUCH1	0.77045112	0.21652554	12.66	0.0004	0.108
EPOSTHS1	0.01939152	0.22458348	0.01	0.9312	0.000
FAMSIZE1	0.09038110	0.05194204	3.03	0.0819	0.034
BRTHORDR	0.01994622	0.04151626	0.23	0.6309	0.000
LANGPRB1	0.02574955	0.48788201	0.00	0.9579	0.000
BLACK	-0.75526787	0.30114501	6.29	0.0121	-0.069
HISPANIC	-0.64316010	0.36507377	3.10	0.0781	-0.035
ASIAN	-2.42861503	0.58088722	17.48	0.0000	-0.131
BOSTON	1.91135015	0.57616870	11.00	0.0009	0.100
FRESNO	0.99007375	0.61259856	2.61	0.1061	0.026
PORTLAND	-0.78468352	0.88747147	0.78	0.3766	0.000
SANDIEGO	3.08008909	0.57748992	28.45	0.0000	0.171

360

Table D.55

LOGIT ANALYSIS OF THE RESTRICTIVE MEASURE OF DROPOUT AFTER SY 1986-87 WITH RACIAL/ETHNIC-TREATMENT INTERACTION TERMS FOR COHORT I YOUTH (ALL SITES INCLUDED)

DEPENDENT VARIABLE: DROPOUT

1311 OBSERVATIONS
1167 DROPOUT = 0
144 DROPOUT = 1
279 OBSERVATIONS DELETED DUE TO MISSING VALUES

VARIABLE	BETA	STD. ERROR	CHI-SQUARE	P	R
INTERCEPT	-0.21243768	1.81429720	0.01	0.9068	
BLACK_T	-0.15514918	0.26139144	0.35	0.5528	0.000
HISP_T	-0.11958523	0.42677523	0.08	0.7793	0.000
ASIAN_T	0.32501368	0.75220390	0.19	0.6657	0.000
OTHER_T	-0.63773335	0.53659170	1.41	0.2346	0.000
STDREAD1	-0.00438384	0.00229113	3.66	0.0557	-0.043
STDMATH1	-0.00024355	0.00187400	0.02	0.8966	0.000
MALE	0.15223871	0.20180202	0.57	0.4506	0.000
ENJREAD1	-0.05206789	0.09941430	0.27	0.6005	0.000
ENJMATH1	-0.06006615	0.09270963	0.42	0.5171	0.000
REPEAT1	0.38946958	0.23365556	2.78	0.0955	0.029
GRADE1	0.06054567	0.15961058	0.14	0.7044	0.000
ABSMUCH1	0.78331800	0.21625442	13.12	0.0003	0.111
EPOSTHS1	0.05133629	0.22436860	0.05	0.8190	0.000
FAMSIZE1	0.08557858	0.05153596	2.76	0.0968	0.029
BRTHORDR	0.02106352	0.04129281	0.26	0.6100	0.000
LANGPRB1	-0.00782656	0.48674619	0.00	0.9872	0.000
BLACK	-0.96276071	0.38732018	6.18	0.0129	-0.068
HISPANIC	-0.86342579	0.45931795	3.53	0.0601	-0.041
ASIAN	-2.82447996	0.71327099	15.68	0.0001	-0.123
BOSTON	1.51586791	0.40568702	13.96	0.0002	0.115
FRESNO	0.54164374	0.43998284	1.52	0.2183	0.000
PORTLAND	-1.24906555	0.68313012	3.34	0.0675	-0.038
SANDIEGO	3.01386837	0.40603493	55.10	0.0000	0.242

Table D.56

LOGIT ANALYSIS OF THE RESTRICTIVE MEASURE OF DROPOUT AFTER SY 1986-87 WITH GENDER-TREATMENT INTERACTION TERMS FOR COHORT I YOUTH (ALL SITES INCLUDED)

DEPENDENT VARIABLE: DROPOUT

1311 OBSERVATIONS
1167 DROPOUT = 0
144 DROPOUT = 1
279 OBSERVATIONS DELETED DUE TO MISSING VALUES

VARIABLE	BETA	STD. ERROR	CHI-SQUARE	P	R
INTERCEPT	-0.09393810	1.82175740	0.00	0.9589	
MALE_T	0.30963854	0.27473774	1.27	0.2597	0.000
FEMALE_T	-0.72109111	0.29262285	6.07	0.0137	-0.067
STDREAD1	-0.00454202	0.00230281	3.89	0.0486	-0.046
STDMATH1	0.00007443	0.00188151	0.00	0.9684	0.000
MALE	-0.32784605	0.27417605	1.43	0.2318	0.000
ENJREAD1	-0.05364402	0.09975586	0.29	0.5907	0.000
ENJMATH1	-0.05637046	0.09277504	0.37	0.5435	0.000
REPEAT1	0.39344024	0.23369573	2.83	0.0923	0.030
GRADE1	0.03928588	0.15916861	0.06	0.8050	0.000
ABSMUCH1	0.75275093	0.21748116	11.98	0.0005	0.105
EPOSTHS1	0.06677956	0.22477288	0.09	0.7664	0.000
FAMSIZE1	0.08340441	0.05154033	2.62	0.1056	0.026
BRTHORDR	0.01729531	0.04145132	0.17	0.6765	0.000
LANGPRB1	0.01237874	0.48163229	0.00	0.9795	0.000
BLACK	-0.75059128	0.30149407	6.20	0.0128	-0.068
HISPANIC	-0.60439078	0.36402349	2.76	0.0969	-0.029
ASIAN	-2.46992938	0.57720561	18.31	0.0000	-0.134
BOSTON	1.53224563	0.40592073	14.25	0.0002	0.116
FRESNO	0.54157835	0.44002780	1.51	0.2184	0.000
PORTLAND	-1.23645018	0.68292585	3.28	0.0702	-0.038
SANDIEGO	3.00888836	0.40648576	54.79	0.0000	0.241

Table D.57

LOGIT ANALYSIS OF THE LESS RESTRICTIVE MEASURE OF DROPOUT AFTER SY 1986-87 FOR COHORT I YOUTH (ALL SITES INCLUDED)

DEPENDENT VARIABLE: DROP2

1311 OBSERVATIONS
992 DROP2 = 0
319 DROP2 = 1
279 OBSERVATIONS DELETED DUE TO MISSING VALUES

VARIABLE	BETA	STD. ERROR	CHI-SQUARE	P	R
INTERCEPT	-0.13438891	1.26822956	0.01	0.9156	
TREAT	-0.17465783	0.13693817	1.63	0.2021	0.000
STDREAD1	-0.00510178	0.00163240	9.77	0.0018	-0.073
STDMATH1	-0.00038705	0.00125481	0.10	0.7577	0.000
MALE	0.13617391	0.13940375	0.95	0.3287	0.000
ENJREAD1	0.03824843	0.06984096	0.30	0.5839	0.000
ENJMATH1	-0.07276475	0.06431618	1.28	0.2579	0.000
REPEAT1	0.36468176	0.16634243	4.81	0.0284	0.044
GRADE1	0.25950210	0.10712553	5.87	0.0154	0.052
ABSMUCH1	0.69707705	0.15000023	21.60	0.0000	0.116
EPOSTHS1	-0.12501737	0.15454492	0.65	0.4186	0.000
FAMSIZE1	0.03787563	0.03455348	1.20	0.2730	0.000
BRTHORDR	0.02566256	0.02858943	0.81	0.3694	0.000
LANGPRB1	-0.12501850	0.32085608	0.15	0.6968	0.000
BLACK	-0.43815337	0.20665811	4.50	0.0340	-0.041
HISPANIC	-0.34265148	0.26978378	1.61	0.2041	0.000
ASIAN	-0.94889552	0.34715955	7.47	0.0063	-0.061
BOSTON	0.66645244	0.25533553	6.81	0.0091	0.058
FRESNO	0.08382322	0.26682516	0.10	0.7534	0.000
PORTLAND	1.30151966	0.23332741	31.12	0.0000	0.141
SANDIEGO	1.52120519	0.24869897	37.41	0.0000	0.156

Table D.58

LOGIT ANALYSIS OF THE LESS RESTRICTIVE MEASURE OF DROPOUT AFTER SY 1986-87 WITH SITE-TREATMENT INTERACTION TERMS FOR COHORT I (ALL SITES INCLUDED)

DEPENDENT VARIABLE: DROP2

1311 OBSERVATIONS
992 DROP2 = 0
319 DROP2 = 1
279 OBSERVATIONS DELETED DUE TO MISSING VALUES

VARIABLE	BETA	STD. ERROR	CHI-SQUARE	P	R
INTERCEPT	-0.19607477	1.27641748	0.02	0.8779	
BOS_T	-0.35101424	0.30756085	1.30	0.2538	0.000
FRESNO_T	-0.51910865	0.33747660	2.37	0.1240	-0.016
PORT_T	-0.21386797	0.27065894	0.62	0.4294	0.000
DIEGO_T	0.22071947	0.28240249	0.61	0.4345	0.000
SEATTL_T	-0.10073587	0.36531311	0.08	0.7827	0.000
STDREAD1	-0.00524749	0.00164017	10.24	0.0014	-0.075
STDMATH1	-0.00025973	0.00125993	0.04	0.8367	0.000
MALE	0.12876507	0.13975624	0.85	0.3569	0.000
ENJREAD1	0.04433061	0.07021452	0.40	0.5278	0.000
ENJMATH1	-0.07659254	0.06459666	1.41	0.2357	0.000
REPEAT1	0.36340891	0.16673256	4.75	0.0293	0.043
GRADE1	0.26305063	0.10743042	6.00	0.0143	0.052
ABSMUCH1	0.69229584	0.15039269	21.19	0.0000	0.115
EPOSTHS1	-0.14153062	0.15533463	0.83	0.3622	0.000
FAMSIZE1	0.04224736	0.03473088	1.48	0.2238	0.000
BRTHORDR	0.02553102	0.02866259	0.79	0.3731	0.000
LANGPRB1	-0.11587604	0.32183308	0.13	0.7188	0.000
BLACK	-0.43291925	0.20735491	4.36	0.0368	-0.040
HISPANIC	-0.33655749	0.27122442	1.54	0.2146	0.000
ASIAN	-0.95059678	0.34902202	7.42	0.0065	-0.061
BOSTON	0.76108790	0.33690044	5.38	0.0204	0.048
FRESNO	0.27162671	0.34832900	0.61	0.4355	0.000
PORTLAND	1.35367690	0.31867633	18.04	0.0000	0.105
SANDIEGO	1.36401859	0.33137918	16.94	0.0000	0.101

Table D.59

LOGIT ANALYSIS OF THE LESS RESTRICTIVE MEASURE OF DROPOUT AFTER SY 1986-87 WITH RACIAL/ETHNIC-TREATMENT INTERACTION TERMS FOR COHORT I YOUTH (ALL SITES INCLUDED)

DEPENDENT VARIABLE: DROP2

1311 OBSERVATIONS
992 DROP2 = 0
319 DROP2 = 1
279 OBSERVATIONS DELETED DUE TO MISSING VALUES

VARIABLE	BETA	STD. ERROR	CHI-SQUARE	P	R
INTERCEPT	-0.19058701	1.27682489	0.02	0.8813	
BLACK_T	-0.15851595	0.17839558	0.79	0.3742	0.000
HISP_T	-0.66545775	0.34905769	3.63	0.0566	-0.034
ASIAN_T	0.30908358	0.41983997	0.54	0.4616	0.000
OTHER_T	-0.05962902	0.36144646	0.03	0.8690	0.000
STDREAD1	-0.00517909	0.00164164	9.95	0.0016	-0.074
STDMATH1	-0.00032796	0.00125774	0.07	0.7943	0.000
MALE	0.11936628	0.14004360	0.73	0.3940	0.000
ENJREAD1	0.03746159	0.06993033	0.29	0.5922	0.000
ENJMATH1	-0.07409699	0.06450718	1.32	0.2507	0.000
REPEAT1	0.37178657	0.16693397	4.96	0.0259	0.045
GRADE1	0.26199513	0.10773775	5.91	0.0150	0.052
ABSMUCH1	0.69799568	0.15064738	21.47	0.0000	0.116
EPOSTHS1	-0.13418713	0.15496571	0.75	0.3865	0.000
FAMSIZE1	0.04003929	0.03469431	1.33	0.2485	0.000
BRTHORDR	0.02597874	0.02866543	0.82	0.3648	0.000
LANGPRB1	-0.15415960	0.32350415	0.23	0.6337	0.000
BLACK	-0.39485964	0.27329848	2.09	0.1485	-0.008
HISPANIC	-0.07683943	0.34534024	0.05	0.8239	0.000
ASIAN	-1.11349748	0.43299360	6.61	0.0101	-0.056
BOSTON	0.67041289	0.25513688	6.90	0.0086	0.058
FRESNO	0.08699961	0.26674400	0.11	0.7443	0.000
PORTLAND	1.31549501	0.23336243	31.78	0.0000	0.143
SANDIEGO	1.52810291	0.24860737	37.78	0.0000	0.157

Table D.60

LOGIT ANALYSIS OF THE LESS RESTRICTIVE MEASURE OF DROPOUT AFTER SY 1986-87 WITH GENDER-TREATMENT INTERACTION TERMS FOR COHORT I YOUTH (ALL SITES INCLUDED)

DEPENDENT VARIABLE: DROP2

1311 OBSERVATIONS
992 DROP2 = 0
319 DROP2 = 1
279 OBSERVATIONS DELETED DUE TO MISSING VALUES

VARIABLE	BETA	STD. ERROR	CHI-SQUARE	P	R
INTERCEPT	-0.08096670	1.27036091	0.00	0.9492	
MALE_T	0.02934695	0.18743346	0.02	0.8756	0.000
FEMALE_T	-0.40783078	0.20133871	4.10	0.0428	-0.038
STDREAD1	-0.00508092	0.00163489	9.66	0.0019	-0.073
STDMATH1	-0.00034713	0.00125597	0.08	0.7823	0.000
MALE	-0.06934608	0.18993151	0.13	0.7150	0.000
ENJREAD1	0.03696026	0.06994870	0.28	0.5972	0.000
ENJMATH1	-0.07248887	0.06436879	1.27	0.2601	0.000
REPEAT1	0.36752699	0.16652924	4.87	0.0273	0.044
GRADE1	0.25885409	0.10725859	5.82	0.0158	0.051
ABSMUCH1	0.69444716	0.15026249	21.36	0.0000	0.115
EPOSTHS1	-0.11499745	0.15492046	0.55	0.4579	0.000
FAMSIZE1	0.03761185	0.03462273	1.18	0.2773	0.000
BRTHORDR	0.02543482	0.02861845	0.79	0.3741	0.000
LANGPRB1	-0.12574220	0.32045718	0.15	0.6948	0.000
BLACK	-0.42292773	0.20719520	4.17	0.0412	-0.039
HISPANIC	-0.31638217	0.27054780	1.37	0.2422	0.000
ASIAN	-0.94923116	0.34753042	7.46	0.0063	-0.061
BOSTON	0.67758579	0.25573404	7.02	0.0081	0.059
FRESNO	0.08573537	0.26693281	0.10	0.7481	0.000
PORTLAND	1.30252774	0.23349094	31.12	0.0000	0.141
SANDIEGO	1.51773179	0.24894018	37.17	0.0000	0.155

Table D.61

LOGIT ANALYSIS OF THE SELF-REPORT MEASURE OF DROPOUT FOR COHORT I YOUTH, FALL 1987 (ALL SITES INCLUDED)

DEPENDENT VARIABLE: ADRPOUT6

1113 OBSERVATIONS
969 ADRPOUT6= 0
144 ADRPOUT6= 1
223 OBSERVATIONS DELETED DUE TO MISSING VALUES

VARIABLE	BETA	STD. ERROR	CHI-SQUARE	P	R
INTERCEPT	-0.05750195	1.83440133	0.00	0.9750	
TREAT	-0.31001092	0.19520306	2.52	0.1123	-0.025
MALE	0.18637060	0.19719515	0.89	0.3446	0.000
ASIAN	-1.23972878	0.53016521	5.47	0.0194	-0.064
BLACK	-0.88345342	0.27006086	10.70	0.0011	-0.101
HISPANIC	0.21545792	0.35006818	0.38	0.5382	0.000
FAMSIZE1	0.02195217	0.04828824	0.21	0.6494	0.000
BRTHORDR	0.10974520	0.03899457	7.92	0.0049	0.083
GRADE1	0.46635012	0.15386383	9.19	0.0024	0.092
ABSMUCH1	0.72734536	0.20264311	12.88	0.0003	0.113
REPEAT1	0.86417757	0.22255899	15.08	0.0001	0.123
EPOSTHS1	-0.30054070	0.20447818	2.16	0.1416	-0.014
LANGPRB1	-0.02807375	0.45697256	0.00	0.9510	0.000
ENJREAD1	-0.04085623	0.09751276	0.18	0.6752	0.000
ENJMATH1	0.11133625	0.08854129	1.58	0.2086	0.000
STDREAD1	-0.00513251	0.00231352	4.92	0.0265	-0.058
STDMATH1	-0.00339397	0.00174157	3.80	0.0513	-0.046
BOSTON	0.35985249	0.30424953	1.40	0.2369	0.000
FRESNO	-0.78055298	0.34268900	5.19	0.0227	-0.061
PORTLAND	-0.38345785	0.31578767	1.47	0.2246	0.000
SANDIEGO	-0.64643926	0.38497856	2.82	0.0931	-0.031

Table D.62

LOGIT ANALYSIS OF THE SELF-REPORT MEASURE OF DROPOUTS WITH SITE-TREATMENT INTERACTION TERMS FOR COHORT I YOUTH, FALL 1987 (ALL SITES INCLUDED)

DEPENDENT VARIABLE: ADRPOUT6

1113 OBSERVATIONS
969 ADRPOUT6= 0
144 ADRPOUT6= 1
223 OBSERVATIONS DELETED DUE TO MISSING VALUES

VARIABLE	BETA	STD. ERROR	CHI-SQUARE	P	R
INTERCEPT	0.12108120	1.85075068	0.00	0.9478	
MALE	0.20761618	0.19839394	1.10	0.2953	0.000
ASIAN	-1.16956074	0.53223602	4.83	0.0280	-0.057
BLACK	-0.84363144	0.27174428	9.64	0.0019	-0.094
HISPANIC	0.28761007	0.35474511	0.66	0.4175	0.000
FAMSIZE1	0.01743085	0.04860894	0.13	0.7199	0.000
BRTHORDR	0.10620204	0.03915504	7.36	0.0067	0.079
GRADE1	0.45298648	0.15408236	8.64	0.0033	0.088
ABSMUCH1	0.75897956	0.20384604	13.86	0.0002	0.118
REPEAT1	0.86538988	0.22386789	14.94	0.0001	0.123
EPOSTHS1	-0.31334881	0.20638335	2.31	0.1289	-0.019
LANGPRB1	-0.06376664	0.45890884	0.02	0.8895	0.000
ENJREAD1	-0.04811988	0.09823098	0.24	0.6242	0.000
ENJMATH1	0.10319500	0.08883838	1.35	0.2454	0.000
STDREAD1	-0.00502782	0.00232046	4.69	0.0303	-0.056
STDMATH1	-0.00361837	0.00175561	4.25	0.0393	-0.051
BOSTON	0.34405607	0.40058231	0.74	0.3904	0.000
FRESNO	-1.05516640	0.46103522	5.24	0.0221	-0.061
PORTLAND	-0.02518222	0.40629113	0.00	0.9506	0.000
SANDIEGO	-0.50535449	0.48608215	1.08	0.2985	0.000
BOST_TRT	-0.20056539	0.36759182	0.30	0.5853	0.000
FRES_TRT	0.25300477	0.43411140	0.34	0.5600	0.000
PORT_TRT	-1.12177152	0.49581911	5.12	0.0237	-0.060
SAND_TRT	-0.56496105	0.60778074	0.86	0.3526	0.000
SEAT_TRT	-0.24174836	0.40540017	0.36	0.5510	0.000

Table D.63

LOGIT ANALYSIS OF THE SELF-REPORT MEASURE OF DROPOUT WITH RACIAL/ETHNIC INTERACTION TERMS FOR COHORT I YOUTH (FALL 1987) (ALL SITES INCLUDED)

DEPENDENT VARIABLE: ADRPOUT6

1113 OBSERVATIONS
969 ADRPOUT6= 0
144 ADRPOUT6= 1
223 OBSERVATIONS DELETED DUE TO MISSING VALUES

VARIABLE	BETA	STD. ERROR	CHI-SQUARE	P	R
INTERCEPT	-0.12765404	1.83775979	0.00	0.9446	
MALE	0.18402476	0.19847251	0.86	0.3538	0.000
ASIAN	-0.98811705	0.58223502	2.88	0.0897	-0.032
BLACK	-1.22180960	0.35870233	11.60	0.0007	-0.106
HISPANIC	0.25400391	0.43254803	0.34	0.5571	0.000
FAMSIZE1	0.01839092	0.04850209	0.14	0.7046	0.000
BRTHORDR	0.10703360	0.03905181	7.51	0.0061	0.080
GRADE1	0.47134009	0.15404828	9.36	0.0022	0.093
ABSMUCH1	0.73623387	0.20369553	13.06	0.0003	0.114
REPEAT1	0.87387460	0.22312132	15.34	0.0001	0.125
EPOSTHS1	-0.28378953	0.20530285	1.91	0.1669	0.000
LANGPRB1	0.03795151	0.46353427	0.01	0.9347	0.000
ENJREAD1	-0.02669687	0.09800784	0.07	0.7853	0.000
ENJMATH1	0.10035632	0.08853748	1.28	0.2570	0.000
STDREAD1	-0.00494702	0.00230205	4.62	0.0316	-0.055
STDMATH1	-0.00336688	0.00174795	3.71	0.0541	-0.045
BOSTON	0.36090217	0.30608045	1.39	0.2384	0.000
FRESNO	-0.76322697	0.34293124	4.95	0.0260	-0.059
PORTLAND	-0.38866010	0.31759671	1.50	0.2210	0.000
SANDIEGO	-0.65335526	0.38683396	2.85	0.0912	-0.032
ASIAN_T	-1.86234249	1.09585265	2.89	0.0892	-0.032
BLACK_T	0.11121187	0.26808729	0.17	0.6783	0.000
HISP_T	-0.73184522	0.42133152	3.02	0.0824	-0.034
OTHER_T	-0.61141145	0.46692859	1.71	0.1904	0.000

Table D.64

LOGIT ANALYSIS OF THE SELF-REPORT MEASURE OF DROPOUT WITH GENDER-TREATMENT INTERACTION TERMS FOR COHORT I YOUTH, FALL 1987 (ALL SITES INCLUDED)

DEPENDENT VARIABLE: ADRPOUT6

1113 OBSERVATIONS
969 ADRPOUT6= 0
144 ADRPOUT6= 1
223 OBSERVATIONS DELETED DUE TO MISSING VALUES

VARIABLE	BETA	STD. ERROR	CHI-SQUARE	P	R
INTERCEPT	-0.06906497	1.83504250	0.00	0.9700	
MALE	0.32906238	0.27061174	1.48	0.2240	0.000
ASIAN	-1.25583283	0.53179487	5.58	0.0182	-0.065
BLACK	-0.89504209	0.27089947	10.92	0.0010	-0.102
HISPANIC	0.19396467	0.35157331	0.30	0.5812	0.000
FAMSIZE1	0.02383460	0.04829211	0.24	0.6216	0.000
BRTHORDR	0.10973347	0.03899366	7.92	0.0049	0.083
GRADE1	0.46616351	0.15368188	9.20	0.0024	0.092
ABSMUCH1	0.73235025	0.20286589	13.03	0.0003	0.113
REPEAT1	0.85810868	0.22275606	14.84	0.0001	0.122
EPOSTHS1	-0.30809678	0.20483714	2.26	0.1326	-0.017
LANGPRB1	-0.02187533	0.45796983	0.00	0.9619	0.000
ENJREAD1	-0.04069338	0.09747108	0.17	0.6763	0.000
ENJMATH1	0.11030282	0.08860978	1.55	0.2132	0.000
STDREAD1	-0.00516162	0.00231512	4.97	0.0258	-0.059
STDMATH1	-0.00345006	0.00174552	3.91	0.0481	-0.047
BOSTON	0.35315080	0.30431260	1.35	0.2459	0.000
FRESNO	-0.78429663	0.34309531	5.23	0.0223	-0.061
PORTLAND	-0.38513006	0.31594704	1.49	0.2229	0.000
SANDIEGO	-0.63695826	0.38550480	2.73	0.0985	-0.029
MALE_T	-0.44398988	0.26138554	2.89	0.0894	-0.032
FEMALE_T	-0.13937296	0.29358292	0.23	0.6350	0.000

Table D.65

LOGIT ANALYSIS OF THE PROBABILITY OF USING CONTRACEPTION DURING LAST INTERCOURSE FOR SEXUALLY ACTIVE COHORT I YOUTH (FALL 1987)

DEPENDENT VARIABLE: CNCONTR6

525 OBSERVATIONS
186 CNCONTR6= 0
339 CNCONTR6= 1
811 OBSERVATIONS DELETED DUE TO MISSING VALUES

VARIABLE	BETA	STD. ERROR	CHI-SQUARE	P	R
INTERCEPT	0.43847110	0.80416042	0.30	0.5856	
TREAT	0.25623568	0.19554631	1.72	0.1901	0.000
AGE1	-0.16314947	0.19713845	0.68	0.4079	0.000
MALE	0.54345965	0.21565234	6.35	0.0117	0.080
BLACK	0.09923218	0.31285377	0.10	0.7511	0.000
HISPANIC	-0.45811921	0.38776859	1.40	0.2374	0.000
FAMSIZE1	-0.09690173	0.05831575	2.76	0.0966	-0.033
FEMHD11	-0.32731879	0.21262118	2.37	0.1237	-0.023
BRTHORDR	0.01001981	0.04123578	0.06	0.8080	0.000
ABSMUCH1	-0.31479074	0.21542513	2.14	0.1439	-0.014
SEXED11	-0.00777179	0.22929244	0.00	0.9730	0.000
SEXEDNR	0.25120174	0.29644742	0.72	0.3968	0.000
EVERSX11	0.09400338	0.24002419	0.15	0.6953	0.000
AKNWLDG1	0.08427751	0.06502183	1.68	0.1949	0.000
AVAILBL1	-0.06306787	0.05707789	1.22	0.2692	0.000
KNOWCON1	-0.05325209	0.14574570	0.13	0.7148	0.000
JSPNSBL1	0.11360139	0.21873511	0.27	0.6035	0.000
BOSTON	-0.36733341	0.32528496	1.28	0.2588	0.000
FRESNO	-0.25166506	0.34515529	0.53	0.4659	0.000
PORTLAND	0.53695284	0.34807763	2.38	0.1229	0.024
SANDIEGO	0.02808231	0.35893283	0.01	0.9376	0.000
CNCONT11	0.23657664	0.46205672	0.26	0.6086	0.000
CNCONTNR	0.28399551	0.42009391	0.46	0.4990	0.000

Table D.66

LOGIT ANALYSIS OF THE PROBABILITY OF USING CONTRACEPTION DURING LAST INTERCOURSE WITH SITE-TREATMENT INTERACTION TERMS FOR SEXUALLY ACTIVE COHORT I YOUTH (FALL 1987)

DEPENDENT VARIABLE: CNCONTR6

525 OBSERVATIONS
186 CNCONTR6= 0
339 CNCONTR6= 1
102 OBSERVATIONS DELETED DUE TO MISSING VALUES

VARIABLE	BETA	STD. ERROR	CHI-SQUARE	P	R
INTERCEPT	0.33410128	0.85623688	0.15	0.6964	
AGE1	-0.15163922	0.19875622	0.58	0.4455	0.000
MALE	0.54562102	0.21666225	6.34	0.0118	0.080
BLACK	0.09212806	0.31402280	0.09	0.7692	0.000
HISPANIC	-0.46855036	0.38909865	1.45	0.2285	0.000
FAMSIZE1	-0.09769126	0.05847986	2.79	0.0948	-0.034
FEMHD11	-0.32272137	0.21349231	2.29	0.1306	-0.020
BRTHORDR	0.01130469	0.04159797	0.07	0.7858	0.000
ABSMUCH1	-0.31551335	0.21609192	2.13	0.1443	-0.014
SEXED11	-0.00870198	0.23028334	0.00	0.9699	0.000
SEXEDNR	0.24611129	0.29694113	0.69	0.4072	0.000
EVERSX11	0.09666628	0.24176692	0.16	0.6893	0.000
AKNWLDG1	0.08237778	0.06521454	1.60	0.2065	0.000
AVAILBL1	-0.06276688	0.05747348	1.19	0.2748	0.000
KNOWCON1	-0.05740597	0.14625771	0.15	0.6947	0.000
JSPNSBL1	0.11009194	0.21920153	0.25	0.6155	0.000
BOSTON	-0.35733094	0.48590351	0.54	0.4621	0.000
FRESNO	-0.01953562	0.49052816	0.00	0.9682	0.000
PORTLAND	0.57807275	0.50766872	1.30	0.2548	0.000
SANDIEGO	0.19977689	0.53503754	0.14	0.7089	0.000
CNCONT11	0.24868677	0.46450788	0.29	0.5924	0.000
CNCONTNR	0.29860906	0.42227477	0.50	0.4795	0.000
BOST_TRT	0.43751331	0.38942238	1.26	0.2612	0.000
FRES_TRT	-0.02173610	0.38003772	0.00	0.9544	0.000
PORT_TRT	0.38602283	0.45657093	0.71	0.3978	0.000
SAND_TRT	0.12933750	0.46866475	0.08	0.7826	0.000
SEAT_TRT	0.43049591	0.51720448	0.69	0.4052	0.000

Table D.67

LOGIT ANALYSIS OF THE PROBABILITY OF USING CONTRACEPTION DURING LAST INTERCOURSE WITH GENDER-TREATMENT INTERACTION TERMS FOR SEXUALLY ACTIVE COHORT I YOUTH (FALL 1987)

DEPENDENT VARIABLE: CNCONTR6

525 OBSERVATIONS
186 CNCONTR6= 0
339 CNCONTR6= 1
102 OBSERVATIONS DELETED DUE TO MISSING VALUES

VARIABLE	BETA	STD. ERROR	CHI-SQUARE	P	R
INTERCEPT	0.39093645	0.81734351	0.23	0.6324	
AGE1	-0.16547850	0.19731821	0.70	0.4017	0.000
MALE	0.61410979	0.30717729	4.00	0.0456	0.054
BLACK	0.09962812	0.31296930	0.10	0.7502	0.000
HISPANIC	-0.46331596	0.38808971	1.43	0.2325	0.000
FAMSIZE1	-0.09746572	0.05834599	2.79	0.0948	-0.034
FEMHD11	-0.32861475	0.21265587	2.39	0.1223	-0.024
BRTHORDR	0.00919782	0.04127781	0.05	0.8237	0.000
ABSMUCH1	-0.31204716	0.21558760	2.10	0.1478	-0.012
SEXED11	-0.01074118	0.22951953	0.00	0.9627	0.000
SEXEDNR	0.25056799	0.29648673	0.71	0.3980	0.000
EVERSX11	0.09378448	0.24007100	0.15	0.6961	0.000
AKNWLDG1	0.08316453	0.06512587	1.63	0.2016	0.000
AVAILBL1	-0.06209912	0.05716534	1.18	0.2773	0.000
KNOWCON1	-0.05337702	0.14579179	0.13	0.7143	0.000
JSPNSBL1	0.12178509	0.22022308	0.31	0.5803	0.000
BOSTON	-0.37251766	0.32567989	1.31	0.2527	0.000
FRESNO	-0.25333596	0.34512474	0.54	0.4629	0.000
PORTLAND	0.53841465	0.34811464	2.39	0.1219	0.024
SANDIEGO	0.03002252	0.35904326	0.01	0.9334	0.000
CNCONT11	0.23654205	0.46161455	0.26	0.6084	0.000
CNCONTNR	0.29219100	0.42046038	0.48	0.4871	0.000
MALE_T	0.20938356	0.24335210	0.74	0.3896	0.000
FEMALE_T	0.33979020	0.32430339	1.10	0.2948	0.000

Table D.68

LOGIT ANALYSIS OF THE PROBABILITY OF EVER HAVING FATHERED A CHILD FOR COHORT I MALES (FALL 1987)

DEPENDENT VARIABLE: FATHCHD6

429 OBSERVATIONS
357 FATHCHD6= 0
72 FATHCHD6= 1
282 OBSERVATIONS DELETED DUE TO MISSING VALUES

VARIABLE	BETA	STD. ERROR	CHI-SQUARE	P	R
INTERCEPT	-3.55786075	1.32007216	7.26	0.0070	
TREAT	0.19223627	0.28511308	0.45	0.5002	0.000
AGE1	-0.58645152	0.29523972	3.95	0.0470	-0.071
BLACK	-0.17901950	0.47456482	0.14	0.7060	0.000
HISPANIC	0.10192558	0.56793479	0.03	0.8576	0.000
FAMSIZE1	0.02843629	0.08228809	0.12	0.7297	0.000
FEMHD11	0.21484233	0.31218692	0.47	0.4913	0.000
BRTHORDR	0.11917178	0.05412868	4.85	0.0277	0.086
ABSMUCH1	0.64165965	0.29467240	4.74	0.0294	0.084
SEXED11	0.10094631	0.34979532	0.08	0.7729	0.000
SEXEDNR	0.43451205	0.40103901	1.17	0.2786	0.000
EVERSX11	1.08552263	1.00011670	1.18	0.2777	0.000
EVERSXNR	1.18840917	0.69917190	2.89	0.0892	0.048
AKNWLDG1	0.07188788	0.09558995	0.57	0.4520	0.000
AVAILBL1	0.06751109	0.08630464	0.61	0.4341	0.000
KNOWCON1	-0.14619817	0.21790431	0.45	0.5023	0.000
JSPNSBL1	-0.18365904	0.31705546	0.34	0.5624	0.000
BOSTON	-0.03468750	0.46010594	0.01	0.9399	0.000
FRESNO	0.05859013	0.46909074	0.02	0.9006	0.000
PORTLAND	-0.84658643	0.49424734	2.93	0.0867	-0.049
SANDIEGO	-0.13030439	0.51547519	0.06	0.8004	0.000
FTHCHD11	1.80487163	0.56688075	10.14	0.0015	0.145
FTHCHDNR	0.25855055	0.89924816	0.08	0.7737	0.000

Table D.69

LOGIT ANALYSIS OF THE PROBABILITY OF EVER HAVING BEEN PREGNANT FOR COHORT I FEMALES (FALL 1987)

DEPENDENT VARIABLE: EVERPRG6

281 OBSERVATIONS
150 EVERPRG6= 0
131 EVERPRG6= 1
344 OBSERVATIONS DELETED DUE TO MISSING VALUES

VARIABLE	BETA	STD. ERROR	CHI-SQUARE	P	R
INTERCEPT	4.56330310	25.85612573	0.03	0.8599	
TREAT	-0.23084600	0.28252800	0.67	0.4139	0.000
AGE1	-0.81649238	0.29046770	7.90	0.0049	-0.123
BLACK	1.13232642	0.45567853	6.17	0.0130	0.104
HISPANIC	1.27075829	0.59903785	4.50	0.0339	0.080
FAMSIZE1	0.06774623	0.08053296	0.71	0.4002	0.000
FEMHD11	-0.29863928	0.30168903	0.98	0.3222	0.000
BRTHORDR	-0.07457983	0.05672499	1.73	0.1886	0.000
ABSMUCH1	0.48638171	0.31255418	2.42	0.1197	0.033
SEXED11	0.10424360	0.35343766	0.09	0.7680	0.000
SEXEDNR	0.56428637	0.45442144	1.54	0.2143	0.000
EVERSX11	-5.60213135	.	.	.	.
EVERSXNR	0.84138320	0.53258610	2.50	0.1142	0.036
AKNWLDG1	0.27541920	0.09800651	7.90	0.0050	0.123
AVAILBL1	-0.21387066	0.08617366	6.16	0.0131	-0.104
KNOWCON1	-0.28750325	0.20715925	1.93	0.1652	0.000
JSPNSBL1	0.34361124	0.32957304	1.09	0.2971	0.000
BOSTON	-0.07524623	0.45998072	0.03	0.8701	0.000
FRESNO	0.63682568	0.52066258	1.50	0.2213	0.000
PORTLAND	0.51986693	0.46696668	1.24	0.2656	0.000
SANDIEGO	-0.52534110	0.50039593	1.10	0.2938	0.000
EVRPRG11	2.51318167	1.09605403	5.26	0.0219	0.092
EVRPRGNR	-6.46714890	.	.	.	.

Table D.70

REGRESSION ANALYSIS OF QUARTERLY EARNINGS FOR SUMMER 1987 FOR COHORT I YOUTH

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DEP VARIABLE: EARNQ9
ANALYSIS OF VARIANCE

SOURCE      DF        SUM OF SQUARES       MEAN SQUARE        F VALUE        PROB>F
MODEL       22          117108606          5323118.45         10.420         0.0001
ERROR      783          400004369          510861.26
C TOTAL    805          517112975

ROOT MSE                714.7456           R-SQUARE           0.2265
DEP MEAN                967.9203           ADJ R-SQ           0.2047
C.V.                    73.84344


PARAMETER ESTIMATES
                           PARAMETER           STANDARD         T FOR H0:
VARIABLE    DF             ESTIMATE            ERROR            PARAMETER=0     PROB > |T|

INTERCEP     1           930.88343          374.29336           2.487          0.0131
TREAT        1           -37.24582356        61.34146997       -0.607          0.5439
MALE         1           109.92193           51.48267212        2.135          0.0331
ASIAN        1          -186.12294          123.60529          -1.506          0.1325
BLACK        1          -182.50457           83.64999347       -2.182          0.0294
HISPANIC     1          -263.97456          107.57827          -2.454          0.0144
STDMATH1     1            -0.08858281         0.44947126       -0.197          0.8438
STDREAD1     1            -0.02741847         0.56719472       -0.048          0.9615
REPEAT1      1            77.05638941        61.27787623        1.257          0.2090
AGE1         1          -118.00956           51.54550642       -2.289          0.0223
ABSMUCH1     1            41.93972247        60.21455386        0.697          0.4863
EPOSTHS1     1            -5.81389191        59.78603820       -0.097          0.9226
FAMSIZE1     1           -13.26443025        13.47938080       -0.984          0.3254
BRTHORDR     1            19.28744593        10.94654446        1.762          0.0785
LANGPRB1     1          -129.45283          112.81048          -1.148          0.2515
FEMHD11      1            59.60914472        57.81761765        1.031          0.3029
FEMHDNR      1            52.10638245       213.71510           0.244          0.8074
BOSTON       1           434.40718           86.72104129        5.009          0.0001
FRESNO       1            97.66573945        90.73615352        1.076          0.2821
PORTLAND     1           -64.03143990        82.35320658       -0.778          0.4371
SANDIEGO     1            18.84408065        88.39066519        0.213          0.8312
JBWKS        1             9.63441210         0.97545657        9.877          0.0001
JB_TREAT     1            -3.69602123         1.47216832       -2.511          0.0123
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Table D.71

REGRESSION ANALYSIS OF WEEKLY WAGES DURING SUMMER 1987 FOR COHORT I YOUTH

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DEP VARIABLE: WKPAYQ9
ANALYSIS OF VARIANCE

SOURCE      DF      SUM OF SQUARES      MEAN SQUARE       F VALUE      PROB>F
MODEL       22         269936.15        12269.82510       3.813        0.0001
ERROR      783        2519464.35         3217.70671
C TOTAL    805        2789400.51

ROOT MSE                56.72483       R-SQUARE          0.0968
DEP MEAN                117.1415       ADJ R-SQ          0.0714
C.V.                    48.42418

PARAMETER ESTIMATES
                        PARAMETER           STANDARD      T FOR H0:
VARIABLE     DF          ESTIMATE              ERROR      PARAMETER=0    PROB > |T|

INTERCEP      1         118.20272        29.70529489         3.979          0.0001
TREAT         1       -5.51455864         4.86828417        -1.133          0.2577
MALE          1        8.40537591         4.08585379         2.057          0.0400
ASIAN         1      -13.87009526         9.80976935        -1.414          0.1578
BLACK         1      -10.08062307         6.63877046        -1.518          0.1293
HISPANIC      1      -18.13508113         8.53780619        -2.124          0.0340
STDMATH1      1        0.02748754         0.03567169         0.771          0.4412
STDREAD1      1       -0.01883107         0.04501465        -0.418          0.6758
REPEAT1       1        3.93053207         4.86323714         0.808          0.4192
AGE1          1      -10.22772705         4.09084055        -2.500          0.0126
ABSMUCH1      1        1.72592499         4.77884797         0.361          0.7181
EPOSTHS1      1       -0.34032965         4.74483939        -0.072          0.9428
FAMSIZE1      1        0.18584277         1.06977313         0.174          0.8621
BRTHORDR      1        0.94989568         0.86875794         1.093          0.2746
LANGPRB1      1       -5.93325121         8.95305363        -0.663          0.5077
FEMHD11       1       -2.90943079         4.58861832        -0.634          0.5262
FEMHDNR       1        8.12163033        16.96121479         0.479          0.6322
BOSTON        1       30.31858248         6.88250009         4.405          0.0001
FRESNO        1        3.89589593         7.20115413         0.541          0.5887
PORTLAND      1       -7.28688865         6.53585269        -1.115          0.2652
SANDIEGO      1       -9.87982762         7.01500757        -1.408          0.1594
JBWKS         1        0.09456780         0.07741581         1.222          0.2222
JB_TREAT      1       -0.15622230         0.11683668        -1.337          0.1816
```